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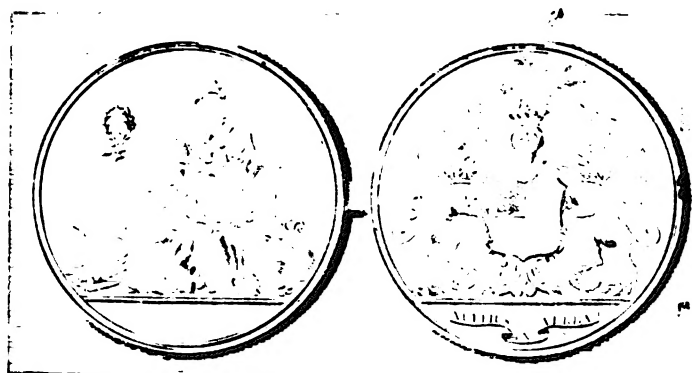




PHILOSOPHICAL  
TRANSACTIONS,  
OF THE  
ROYAL SOCIETY  
OF  
LONDON.

VOL. LXXIII. For the Year 1783.

PART I.



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## A D V E R T I S E M E N T.

**T**HE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought adviseable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society ; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices ; which in some instances have been too lightly credited, to the dishonour of the Society.



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## A P P E N D I X.

*Translation of Count Francesco Ippolito's Letter to Sir William Hamilton, Knight of the Bath, F. R. S. ; giving an Account of the Earthquake which happened in Calabria, March 28, 1783. See p. 209.*

**T**HAT part of the kingdom of Naples, formerly possessed by the Brutii, and other Greek colonies, and now called Calabria, has been at all times exposed to the terrible convulsions, of which we are at present the victims. The earthquakes in 1638 and 1659, by which the two provinces of Calabria were almost utterly destroyed, are fresh in every one's memory, as well as that of the year 174 $\frac{1}{2}$ , which afflicted us for a long time, but without loss of cities or of men. Reggio, and the countries near it, are exposed to earthquakes almost every year, and if we look back to highest antiquity, we shall find that all Italy, but particularly this country, and more particularly still the provinces we inhabit, have been subject to various catastrophes in consequence of volcanoes and subterraneous fires. Indeed, the religious rites themselves of our ancestors the Brutii, which history teaches us were all of a gloomy melancholy cast, attest the deep impression which the sense of such repeated and terrible catastrophes made upon the

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people

people exposed to them. Neither, however, could it, nor can it, be otherwise in countries such as these are, which are intersected by the chain of the Appennines, the bowels of which contain nothing but sulphur, iron, fossil coals, petroleum, and other bituminous and combustible matters. The quantity of these minerals must necessarily occasion fermentations and subterraneous fires, and it is good for us that we have so many volcanoes in the neighbourhood, to serve as chimnies, and afford outlets to the fire which forms under our feet.

But amongst so many earthquakes to which we have been exposed, the least is not that under which we at present suffer, whether we consider the force of the concussions, or their duration, or the changes that have taken place in the surface of the earth, or the ruin of so many cities and villages, with the loss of forty thousand inhabitants.

I have kept a regular account from the day of the first shock of the fifth of February, not only of the convulsions suffered by the earth, but likewise of all the meteors observed in the atmosphere. This the shortness of time will not allow me to transmit to your excellency; but the sum of it is, that from the 5th of February to this instant the shocks have been *more* frequent, and almost every day repeated. At times the earth shook as it usually does on these occasions; but at others the motion was undulatory, and at others vorticoſe, during which last state it resembled a ship tossed about in a high sea. The most considerable of these repeated earthquakes were those which took place on the fifth of February, at 19½ Italian time; on the seventh, about 20½; on the twenty-eighth, about 8½ of the night; and finally on the twenty-eighth of March, about 1½ in the evening. These four eruptions coming, as nearly as we can judge by the phenomena and effects, from the chain of mountains which extend from Reggio hitherwards, have produced four different explosions in four different parts of Calabria. The three former were in that part of the province in which your excellency now is, and that which you must pass through in your journey to Messina. These explosions have produced various great effects; ruined cities and villages, levelled

levelled mountains, immense breaks in the earth, new collections of waters, old rivulets sunk in the earth and dispersed, rivers stopped in their course, soils levelled, small mountains which existed not before formed, plants rooted up, and carried to considerable distances from their first site, large portions of earth rolling about through considerable districts, animals and men swallowed up by the earth—but I abstain from entering into a minute account of these disasters; your excellency will see them with your own eyes, and assisted by the relations of ocular and faithful witnesses, no doubt, form a faithful history of them. One thing, however, I may not forbear to communicate, and that is, that of all these calamities the greatest and most extraordinary was that which happened on the banks of Scilla and Bagnara. That part of the sea which considerably overflowed in these marshes, and swallowed up a great number of people who had taken refuge there, was so hot that it scalded several of those who were saved. This I had from the mouth of the most excellent Vicar General.

But I will confine myself to a short narrative of the effects of the last explosion of the twenty-eighth of March, which, without a doubt, must have arisen from an internal fire in the bowels of the earth in these parts, as it took place precisely in the mountains which cross the neck of our peninsula which is formed by the two rivers, the Lameto which runs into the gulph of St. Euphemia, and the Corace, which runs into the Ionian sea, and properly into the bay of Squillace. That the thing was so is evident from all the phenomena.

This shock, like all the rest, came to us in the direction of the S.W. At first the earth began to undulate, then it shook, and finally it moved in a vorticoſe direction, so that many persons were not able to stand upon their feet. This terrible concussion lasted about ten seconds; it was succeeded by others which were less strong, of less duration, and only undulatory, so that, during the whole night, and for half the next day,



the earth was continually shaken, at first every five minutes, afterwards every quarter of an hour.

A terrible groan from under ground preceded this convulsion, lasted as long as it did, and finally ended with an intense noise, like the thunder of a mine that takes effect. These mighty thunderings accompanied not only the shocks of that night and of the succeeding day, but all the others which have taken place since that time: moreover, groans have sometimes been heard without any shakes of the earth, and prior to the twenty-eighth of March there were noises and crackings which exactly resembled the bursting of so many bombs.

The air was covered with clouds, and the westerly gales blew very fresh. These were stilled in one minute before the horrid crash; but in one moment after they blew again, and then were still. There were, however, frequent and sudden changes of the atmosphere during the whole night, the heavens being alternately cloudy and serene, and different winds blowing, though they all came from between south-west.

At the time of the earthquake, during the night, flames were seen to issue from the ground in the neighbourhood of this city towards the sea, where the explosion extended, so that many countrymen ran away for fear; these flames issued exactly from a place where some days before an extraordinary heat had been perceived.

After the great concussion there appeared in the air, towards the east, a whitish flame, in a slanting direction; it had the appearance of electric fire, and was seen for the space of two hours.

In consequence of the terrible shock, many countries and cities, especially those situated in the neighbourhood and neck of our peninsula as you go from Tiriolo to the river Angitola, and which had suffered nothing before, were overturned. Curinga, Maida, Cortale, Girifalco, Borgia, St. Floro, Settignano, Marcellinara, Tiriolo, and other countries of less importance, were almost entirely destroyed, but with the loss of very few people. Many hundreds, however, perished in Maida, Cortale, and Borgia.

The

The same effects which took place in the country your excellency is now in were likewise produced by the earthquake in these parts. Many hills were divided or laid level; many apertures were made in the surface of the earth throughout the whole surface which lies between the two vallies occupied by the rivers Corace and Lameto, as you go towards Angitola. Out of many of these apertures a great quantity of water coming either from the subterraneous concentrations, or the rivers themselves in the neighbourhood of which the ground broke up, spouted during several hours. From one of these openings in the territory of Borgia, distant about a mile from the sea, there came out a large quantity of salt water which imitated the motions of the sea itself for several days. Warm water likewise issued from the apertures made in the plains of Maida; but I cannot say whether this was of a mineral quality, or heated by the same subterraneous fire.

We must likewise take notice, that there came from the same fissures out of which the water issued some very thin earth, either of a white, grey, or yellow sort, which from its extreme tenuity had all the appearance of a true sand. I have seen only the grey, in which there was evidently a mixture of iron.

It has also been observed, that in all the sandy parts, where the explosion took place, there were observed, from distance to distance, apertures in the form of an inverted cone, out of which likewise there came water. This seems to prove that from thence escaped a flake of electric fire. Fissures of this kind are particularly met with along the banks of the Lameto from the place where it goes into the sea hitherwards for many a mile.

Amidst the various phenomena, which either preceded or followed the earthquake, the two former are remarkable. On the very day of the earthquake the water of a well in Maida, which heretofore people used to drink, was infected with so disgusting a sulphureous taste, that it was impossible even to smell to it. On the other hand, at Catanzaro the water of a well, which before could not be used because of a smell of calcination that

it had, became so pure as to be drunk extremely well. In Maida itself many fountains were dried up by the earthquake of the twenty-eighth. This likewise happened at other places; but many also broke out in several spots where there had been none before, as did also several mineral springs, of which before there was not a vestige. This happened at Cropani, a country of the Marchesato. Commonly, however, the fountains became more swelled and more copious, and emitted a larger volume of water than usual.

The waters of some fountains were also observed to be troubled, and to assume a whitish or yellowish colour, according to the countries through which they passed.

Many elevations of soil likewise took place in consequence of the earthquake. The most notable was that which happened in the bed of the river of Borgia, where there was seen a new hillock, about ten palms high, about twenty palms at the base, and about two hundred palms long. Finally, in the neighbourhood of the river Lameto, and precisely in the district of the country called Amato, which was entirely torn up by the earthquake, there is an olive ground, the surface of which is turned over in a vorticoſe direction; a phenomenon which likewise obtained in many other parts of the country.

Such are the most notable phenomena of the earthquake of the twenty-eighth of March in these countries which have hitherto reached my notice. I think myself, however, obliged to notice to your excellency, that this extraordinary catastrophe of our afflicted province was preceded by great and extraordinary frosts in the winter of 1782; by an extraordinary drought and insufferable heats in the spring of the same year; and by great, copious, and continued rains, which began in autumn, and continued to the end of January. These rains were accompanied by no thunder or lightning, nor were any winds hardly ever heard in these cities where they are used to blow very fresh during all this time; but at the beginning of the earthquake they all seemed to break loose again together, accompanied with hail and rain. For a long time before the earthquake, the sea appeared considerably agitated, so as to frighten the

the fishermen from venturing upon it, without there being any visible winds to make it so. Our volcanoes too, as I am confidently assured, emitted no eruptions for a considerable time before; but there was an eruption of Etna in the first earthquake, and Stromboli shewed some fire in the last. God grant that the pillars of the earth may be again fastened, and the equilibrium of both natural and moral things restored!

I have the honour to be, &c.



PHILOSOPHICAL  
TRANSACTIONS.

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I. *A Letter from William Herschel, Esq. F. R. S.*

TO SIR JOSEPH BANKS, BART. F. R. S.

SIR,

**B**Y the observations of the most eminent Astronomers in Europe it appears, that the new star, which I had the honour of pointing out to them in March, 1781, is a Primary Planet of our Solar System. A body so nearly related to us by its similar condition and situation, in the unbounded expanse of the starry heavens, must often be the subject of the conversation, not only of astronomers, but of every lover of science in general. This consideration then makes it necessary to give it a name, whereby it may be distinguished from the rest of the planets and fixed stars.

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In the fabulous ages of ancient times the appellations of Mercury, Venus, Mars, Jupiter, and Saturn, were given to the Planets, as being the names of their principal heroes and divinities\*. In the present more philosophical æra, it would hardly be allowable to have recourse to the same method, and call on Juno, Pallas, Apollo, or Minerva, for a name to our new heavenly body. The first consideration in any particular event, or remarkable incident, seems to be its chronology: if in any future age it should be asked, *when* this last-found Planet was discovered? It would be a very satisfactory answer to say, "In the Reign of King George the Third." As a philosopher then, the name of GEORGIUM SIDUS presents itself to me, as an appellation which will conveniently convey the information of the time and country where and when it was brought to view. But as a subject of the best of Kings, who is the liberal protector of every art and science;—as a native of the country from whence this Illustrious Family was called to the British throne;—as a member of that Society, which flourishes by the distinguished liberality of its Royal Patron;—and, last of all, as a person now more immediately under the protection of this excellent Monarch, and owing every thing to His unlimited bounty;—I cannot but wish to take this opportunity of expressing my sense of gratitude, by giving the name Georgium Sidus,

*Georgium Sidus*

— *jam nunc assuesce vocari.*

VIRG. Georg.

to a star, which (with respect to us) first began to shine under His auspicious reign.

\* M. DE LA LANDE'S Aft. § 639.

By



*Mr. HERSCHEL on the Georgium Sidus.*

3

By addressing this letter to you, SIR, as President of the Royal Society, I take the most effectual method of communicating that name to the Literati of Europe, which I hope they will receive with pleasure. I have the honour to be, with the greatest respect,

S I R,

Your most humble

and most obedient servant,

W. HERSCHEL.

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II. *On the Diameter and Magnitude of the Georgium Sidus; with a Description of the dark and lucid Disk and Periphery Micrometers.* By William Herschel, Esq. F. R. S.

Read November 7, 1782.

**I**T is not only of the greatest consequence to the astronomer, but also gives the highest pleasure to every intelligent person, to have a just idea of the dimensions of the solar system; and the heavenly bodies that belong to it. As far then as they fall within the reach of our instruments, they ought carefully to be examined and measured by all the various methods we can invent. Almost every sort of micrometer is liable to some inconveniences and deceptions: it will, however, often happen, that we may correct the errors of one instrument by the opposite defects of another. The measures of the diameter of the Georgium Sidus, which were delivered in my first paper, differ considerably from each other. However, if we set aside the three first, on a supposition (as I have hinted before) that every minute object, which is much smaller than what we are frequently used to see, will at first sight appear less than it really is; and take a mean of the remaining observations, we shall have

have  $4''\ 36\frac{1}{4}'''$  for the diameter of the planet. On comparing the measures then with this mean, we find but two of them that differ somewhat more than half a second from it; the rest are almost all within a quarter of a second of that measure. This agreement, in the dimensions of any other planet, would appear very considerable; but not being satisfied, when I thought it possible to obtain much more accurate measures, I employed the lamp-micrometer in preference to the former. The first time I used it upon this occasion I perceived, that if, instead of two lucid points, we could have an intire lucid disk to resemble the Planet, the measures would certainly be still more complete. The difficulty of dilating and contracting a figure that should always remain a circle, appeared to me very considerable, though nature, with her usual simplicity, holds out to us a pattern in the Iris of the eye, which, simple as it appears, is not one of the least admirable of her inimitable works. However, I recollected, that it was not absolutely requisite to have every insensible degree of magnitude; since, by changing the distance, I could without much inconvenience make every little intermediate gradation between a set of circles of a proper size, that might be prepared for the purpose. Intending to put this design into practice, I contrived the following apparatus.

A large lanthorn, of the construction of those small ones that are used with my lamp-micrometer\*, must have a place for three flames in the middle, which is necessary, in order that we may have the quantity of light required, by lighting one, two, or all of them. The grooves, instead of brass sliding doors, must be wide enough to admit a paste-board, and three or four thicknesses of paper. I prepared a set of circles, cut

put in paste-board, increasing by tenths of an inch from two inches to five in diameter, and these were made to fit into the grooves of the lamp. A good number of pieces, some of white, others of light blue paper, of the same size with the paste-boards, were also cut out, and several of them oiled, to render them more transparent. The oiled papers should be well rubbed, that they may not stain the dry papers when placed together. This apparatus being ready, we are to place behind the paste-board circle, next to the light, one, two, or more, either blue or white, dry or oiled, papers; and by means of one or more flames, to obtain an appearance perfectly resembling the disk we would compare it with. It will be found, that more or less altitude of the object, and higher or lower powers of the instrument, require a different assortment of papers and lights, which must by no means be neglected: for if any fallacy can be suspected in the use of this apparatus, it is in the degree of light we must look for it. In a few experiments I tried with these lucid disks, where I placed several of them together, and illuminated them at once, it was found, that but very little more light will make a circle appear of the same size with another, which is one, or even two-tenths of an inch less in diameter. A well known and striking instance of this kind of deception is the moon, just before or after the conjunction, where we may see how much the luminous part of the disk projects above the rest.

The method of using the artificial disks is the same which has been described with the lamp-micrometer, of which this apparatus may be called a branch. We are only to observe, that the Planet we would measure should be caused to go either just under, or just over, the illuminated circle. It may indeed

also

also be suffered to pass across it; but in this case, the lights will be so blended together, that we cannot easily form a proper judgement of their magnitudes. By a good screw to the motions of my telescope I have been able, at any time, to keep the Planet opposite the lucid disk for five minutes together, and to view them both with the most perfect and undisturbed attention. The apparatus I employed being now sufficiently explained, several alterations that were occasionally introduced will be mentioned in the observations and experiments on the Georgium Sidus, as they follow, in the order of time in which they were made.

*Observations on the Light, Diameter, and Magnitude, of the Georgium Sidus.*

Oct. 22, 1781. The Georgium Sidus was perfectly defined with a power of 227; had a fine, bright, steady light; of the colour of Jupiter, or approaching to the light of the Moon.

Nov. 28, 1781. I measured the diameter of the Georgium Sidus by the lamp-micrometer, and took one measure, which I was assured was too large; and one, which I was certain was too little; then taking the mean of both, I compared it with the diameter of the star, and found it to agree very well.

Hence  $\frac{\text{Image} = 2.4 \text{ inches}}{\text{Distance} = 431 \text{ inches}} = \text{tang.}, 0055684$ ; and  $\frac{\text{Angle} = 19' 8''}{\text{Power} = 227.6} = \text{the diameter } 5''.06$ . But the evening was foggy, and the star having much aberration, I was induced to try the above method of extreme and mean diameters, suggested by, the

the method of altitudes; where two equally distant extremes give us a true mean.

Nov. 19, 1781. The diameter measured  $32\frac{1}{2}$  parts of my micrometer, the wires being outward tangents to the disk. On shutting them gradually by the same light, they closed at 24; therefore the difference is  $8\frac{1}{2}$  parts, which, according to my scale, gives  $5'' 2'''$  for the diameter. This was taken with 227, and the measure seemed large enough. Not perfectly pleased with my light, which was rather too strong, I repeated the measure, and had  $33\frac{1}{2}$  parts; then shutting the wires gradually, by *this* light they closed at 25: the difference, which is  $8\frac{1}{2}$  parts, gives  $5'' 11'''$ .

Aug. 29, 1782. 15 h. I saw the Georgium Sidus full as well defined with 460, as Jupiter would have been at that altitude with the same power.

Sept. 9, 1782. Circumstances being favourable, I took a measure of the diameter of the Georgium Sidus with the power of 460, and silk-thread micrometer. After a proper allowance for the zero, I found  $4'' 11'''$ .

Oct. 2, 1782. I had prepared an apparatus of lucid disks, and measured the diameter of the Georgium Sidus with it. Having only white oiled papers, I placed two of them together, and used only a single lamp; but could not exactly imitate the light of the Planet. When I first saw the Sidus and luminous circle together, I was struck with the different colours of their lights; which brought to my recollection  $\gamma$  Andromedæ,  $\epsilon$  Bootis,  $\alpha$  Herculis,  $\beta$  Cygni, and other coloured stars. The Planet unexpectedly appeared blueish, while the lucid disk had a strong tincture of red; but neither of the colours were so vivid and sparkling as those of the just mentioned stars.

The distance of the luminous circle from the eye (which I always measure with deal rods) was 588,25 inches. The circle measured 2,35 inches. Hence we have the angle  $13' 44''$ ; which, divided by the power 227, gives  $3'',63$  for the diameter of the Planet. I suspected some little fallacy from the want of a perfect resemblance in the light and colour of the artificial disk to the real appearance of the Planet.

Oct. 4, 1782. I measured the diameter of the Georgium Sidus again, by an improvement in my apparatus, for I now used pale blue papers, both oiled and plain, instead of white; by which means I obtained a resemblance of colours; and by an assortment of one oiled and two dry papers with two lamps burning, I effected the same degree of light which the Planet had, and both figures were equally well defined. By first changing the disk, and, when I had one which came nearest, changing my distance, I came at a perfect equality between the Planet and disk. The measure was several times repeated with great precaution. The result was  $\frac{2,8}{692,6} = ,0040283$ ; and  $\frac{13' 53'',85}{227} = 3'',67$ . If any thing be wanting to the perfection of this measure, it is perhaps that the Sidus should be in the meridian, in order to have all the advantages of light and distinctness.

Oct. 10, 1782. The measures of the Planet by the lucid disk micrometer appearing to me very small, I resolved to ascertain the power of my telescope again most scrupulously, by an actual experiment, without any deduction from other principles. On a most convenient and level plain I viewed two slips of white paper, and measured their images upon a wall. The distances were measured by deal rods, every repetition whereof



was certainly true to half a tenth of an inch; nor did the direction of the measure ever deviate, so much as two inches, from a straight line.

Distance of the object from the eye in inches	7255,5
Distance of the eye from the vertex of the speculum	80,2
Distance of the vertex of the speculum from the object	7335,7
Distance of the eye from the wall	2292,35
Diameter of the largest paper	,99125
Diameter of the smallest	,5075
Image of the largest paper on the wall	73,
Image of the smallest on the same	37,8
Angle subtended by the large paper at the vertex of the speculum	27'',87
Angle subtended by its image on the wall, at the eye	1° 49' 26'',4.
Power of the telescope deduced from the large paper	235,6
Angle subtended by the small paper at the vertex of the speculum	14'',27.
Angle subtended by its image on the wall, at the eye,	56' 40'',9,
Power of the telescope deduced from the small paper	238,3
Mean of both experiments, as being equally good	237,
Focal length of the speculum upon those objects	86,1625
Upon Capella	85,2
And 237 diminished in the ratio of 85,2 to 86,1625 gives 234,3 for the power of the instrument upon the fixed stars.	

It

It appears then, from these experiments, that the power of the telescope has not been over-rated; and that, therefore, the measures of the Georgium Sidus cannot be found too small on that account.

There is one cause of inaccuracy or deception in very small measures, long suspected, but never yet sufficiently investigated. That there is a *dispersion* of the rays of light in their passage through the atmosphere, we may admit from various experiments; if then the quantity of this dispersion be, in general, regulated by certain dispositions of the air, and other causes, it will follow, that a *concentration* may also take place: for should the rays of light, at any time, be less dispersed than usual, they might with as much reason be said to be concentrated, as the mercury of a thermometer is said to be contracted by cold, when it falls below the zero.

Oct. 12, 1782. The night was so fine, that I saw the Georgium Sidus very plainly with my naked eye. I took a measure of its diameter by the lucid disk, and found, that I was obliged to come nearer, as the Planet rose higher, and gained more distinct light. At the altitude of  $52^{\circ}$  it was as follows:

$$\frac{3.415}{731.3} = .0046698; \text{ and } \frac{16'3'',2}{227} = 4'',24.$$

Oct. 13, 1782. 16 h. I viewed the Georgium Sidus with several powers. With 227 it was beautiful. Still better with 278. With 460, after looking some time, very distinct. I perceived no flattening of the polar regions, to denote a diurnal motion; though, I believe, if it had had as much as Jupiter, I should have seen it. With 625 pretty well defined.

Oct. 19, 1782. The inconvenience arising from the quantity of light contained in the lucid disk, suggested to me the

idea of taking only an illuminated periphery, instead of the area of a circle. By this means I hoped to see the circle well defined, and yet have but little light to interfere with the appearance of the Planet. The breadth of my lucid periphery was one-twentieth of an inch. The result of this measure proved  $\frac{3,3}{765,45} = ,0041486$ ; and  $\frac{14' 15'',69}{227} = 3''77$ .

Oct. 26, 1782. In my last experiment I found the lucid periphery much broader than I could have wished; therefore, I prepared one of no more than one-fortieth part of an inch in breadth, the outer circle measuring very exactly 4,00, and the inner circle 3,95. With this slender ring of light illuminated with only one single lamp, I measured the Georgium Sidus, by removing the telescope to various distances; and found at last the following result:  $\frac{4}{1033,05} = ,0038720$ ; and  $\frac{13' 18'',6}{227} = 3'',51$ .

Nov. 4, 1782. I was now fully convinced that light, be it in the form of a lucid circle, or illuminated periphery, would always occasion the measures to be less than they should be, on account of its vivid impresson upon the eye, whereby the magnitude of the object, to which the Planet was compared, would be increased. It occurred to me then, that if a lucid circle encroached upon the surrounding darker parts, a lucid square border, round a dark circle, would in its turn advance upon the artificial disk. In my last measures, where the Planet had been compared to a lucid ring, I had plainly observed that the Sidus, which was but just equal to the illuminated periphery, was considerably larger than the black area contained within the ring. This seemed to point out a method to discover the quantity of the deception arising from the illumination; and consequently, to furnish us with a correction applicable to such measures;

tures; which would be *plus*, when taken with a lucid disk or ring; and *minus*, when obtained from a dark ring or circle. Having suspended a row of paste-board circles against an illuminated sheet of oiled paper, I caused the Georgium Sidus to pass by them several times, and selected from their number that to which the Planet bore the greatest resemblance in magnitude. I produced a perfect equality by some small alteration of my distance, and the result was as follows:

$$\frac{3,165}{633,95} = ,0049925 : \text{hence } \frac{17' 9'',8}{227} = 4'',53.$$

I was desirous of seeing what would be the effect of lessening the light of the illuminated frame, against which the dark disks were suspended, and also waited a short time that the Planet might rise up higher. The measure being then repeated at a different distance, and with a different black disk, I obtained the following particulars:

$$\frac{3,59}{803,05} = ,0044704 ; \text{ and } \frac{15' 22'',1}{227} = 4'',06.$$

I intend to pursue these experiments still farther, especially in the time of the Planet's opposition, and am therefore unwilling as yet to draw a final conclusion from the several measures. In a subject of such delicacy we cannot have too many facts to regulate our judgement. Thus much, however, we may in general surmise, that the diameter of the Georgium Sidus cannot well be much less, nor perhaps much larger, than about four seconds. From this, if we will anticipate more exact calculations hereafter to be made, we may gather that the real diameter of that planet must be between four and five times that of the earth: for by the calculations of M. DE LA LANDE, contained in a letter he has favoured me with, the distance of the Georgium Sidus is stated at 18,913, that of the earth

earth being 1. And if we take the latter to be seen, at the sun, under an angle of  $17''$ , it would subtend no more than  $,''898$ , when removed to the orbit of the Georgium Sidus.

Hence we obtain  $\frac{4}{,898} = 4,454$ ; which number expresses how much the real diameter of the Georgium Sidus exceeds that of the earth.

III. *Conclusion of the Experiments and Observations concerning the Attractive Powers of the Mineral Acids.* By Richard Kirwan, Esq. F. R. S.

Read Dec. 12, 1782.

HAVING found, as exactly as I was able, the quantity of each of the mineral acids taken up at the point of saturation by alkalies and earths, and also that taken up by phlogiston, when these acids are by it converted into an aërial form, I next endeavoured to find how much of these acids was taken up at the point of saturation by each of the metallic substances, and for this purpose procured the most saturated solution possible of each metallic substance soluble in any of these acids. These solutions did not, indeed, immediately answer my purpose, as they constantly retained an excess of acid; yet as they were the foundation of my subsequent observations, and as the experiments themselves are in many respects useful to be known, I shall here briefly relate their result, and confine myself to those circumstances singly that relate to my future investigations, or that have not heretofore been satisfactorily explained. The acids I used were dephlogisticated so far as to be colourless; the metals were for the most part very fine filings, or reduced in a mortar to a fine powder. They were added little by little to their respective menstrua, much more being thus dissolved than if the whole was thrown in at once; and the solution was performed in glass phials with bent tubes.

*Solution*

*Solution of iron in the vitriolic acid.*

100 grs. of bar-iron, in the temperature of  $56^{\circ}$ , require for their solution, 190 grs. of real acid, whose proportion to that of the water with which it should be diluted, is as 1 to 8, 10, or 12. It would act on iron, though its proportion were greater or lesser, but not so vigourously. If towards the end a heat of  $200^{\circ}$  were applied, 123 grs. of real acid would be sufficient.

The air produced by this solution is intirely inflammable, and generally amounts to 155 cubic inches.

Iron is also soluble with the assistance of a strong heat, and in smaller quantity in concentrated vitriolic acid; and in this case scarce any inflammable air is produced, but a large quantity of vitriolic air as Dr. PRIESTLEY has observed, and a small quantity of sulphur sublimes at the latter end. This fact is a clear refutation of Mr. LAVOISIER's hypothesis; for is it not evident, that the same substance which, when a dilute acid is used, goes off in the form of inflammable air, does when a concentrated acid is used, to unite this acid, and thus form both vitriolic air and sulphur? In the first case it cannot unite to the acid, by reason of the large quantity of water combined with the acid; and as the liquor, being mostly aqueous, contains a large quantity of specific fire, it receives that fire when the acid unites to the metallic earth, and flies off in the form of air. But in the second case, the concentrated acid, containing much less specific fire, cannot expel the phlogiston in the form of inflammable air (as this air absorbs a vast quantity of fire) but unites to it, when by heat it is further stripped of its water, and thus forms both

vitriolic air and sulphur. 100 grs. of iron, dissolved without heat, afford upwards of 400 of vitriol.

100 grs. of the vitriol crystallized contain 25 of iron, 20 of real acid, and 55 of water. When calcined nearly to redness these crystals lose about 40 of water.

The calces of iron are more or less soluble in this acid according to their degree of dephlogistication. Those that are phlogisticated (as that recently precipitated from a solution of vitriol by fixed alkalies) are also most soluble, and upon evaporation afford crystals, though paler than those formed of genuine iron. Those that are least phlogisticated are also least soluble, that is, require more real acid for their solution, and afford no crystals, but only a magma or mother liquor. Hence also, solutions of iron newly made diminish, and consequently phlogisticate the super-incumbent air, and consequently gradually emit phlogiston: and hence the calx, being more dephlogisticated, gradually falls unless more acid be added to keep it in solution.

*Iron in the nitrous acid.*

100 grs. of iron, to be perfectly dissolved, and not barely calcined, require 142 grs. of real nitrous acid, so diluted as that its proportion to water should be as 1 to 13 or 14; and when this last proportion is used, the heat of a candle may be applied for a few seconds, and the access of common air prevented. In this case not above 18 cubic inches of nitrous air are produced, all the rest is absorbed by the solution, and no red vapours appear. But if the proportion of acid and water be as 1 to 8 or 10, and heat be applied, a much greater quantity of iron will be dephlogisticated, though very little of it be held in solution; and by this means I have obtained from 100 grs. of iron, 83,87 cubic inches



of nitrous air; and by distilling the solution a still greater quantity may be obtained, which was absorbed by the solution. No inflammable air is obtained from solutions of iron or any other metallic substance in the nitrous acid, because this acid has less affinity to water, and more to phlogiston, than the vitriolic acid, and also contains much less fire than either the vitriolic or marine acids, as will be seen in the sequel, and therefore unites to phlogiston instead of barely expelling it. And hence it is, that the vitriolic acid, though united with 30 times its weight of water, will still visibly act on iron, and separate inflammable air in the temperature of  $55^{\circ}$ ; whereas nitrous acid, diluted with 15 times its weight of water, will have no visible effect on iron in that temperature.

The calces of iron, if not too much dephlogisticated, are also soluble, though difficultly, in the nitrous acid.

*Iron in the marine acid.*

100 grs. of iron require 215 of real marine acid for their solution. The proportion of acid to that of water in the spirit of salt I used was as 1 to 4. When it is as 1 to 4, it effervesces too violently. Heat is rather prejudicial, as it volatilises the acid. No marine air flies off, and the quantity of inflammable air is just the same as if dilute vitriolic acid were used.

The calces of iron are also soluble in marine acid. They may be distinguished from genuine iron in this, that their colour, when precipitated by fixed alkalies, is *reddish*, whereas the precipitate of genuine iron is *greenish*.

*Copper in the vitriolic acid.*

100 grs. of copper require nearly 183 grs. real vitriolic acid for their solution. The proportion of acid to that of water  
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being

being as 1 to  $\frac{1}{2}$ , or at least as 1 to  $\frac{1}{3}$ , and a strong heat must also be applied. I could never dissolve the whole of any quantity of copper; but to dissolve a given quantity of it, a still greater must be used in the proportion of nearly 28 to 100, though this residuum also is soluble by adding more acid. When copper has been dephlogisticated in this manner, a solution of it is obtained by adding warm water to the dephlogisticated mass.

The dephlogistication of 128 grs. of copper treated in this manner affords 11 cubic inches of inflammable air, and nearly 65 of vitriolic air. When I obtained inflammable the acid was a little more aqueous. The reason why copper cannot be dephlogisticated by dilute vitriolic acid, nor even by the concentrated, without the assistance of a strong heat, as iron is, appears deriveable from its much stronger attraction to phlogiston, and the much greater quantity of it which copper contains, as will hereafter be seen. Hence

100 grs. of vitriol of copper contain 27 of copper, 30 of acid, and 43 of water, of which it loses about 28 by evaporation or slight calcination.

The solution of 100 grs. of copper affords 373 of blue vitriol.

#### *Copper in nitrous acid.*

100 grs. of copper require 130 of real nitrous acid to dissolve them. If the acid be so far diluted as that its proportion to that of water be as 1 to 14, the assistance of heat will be necessary, otherwise not. This solution affords  $67\frac{1}{2}$  cubic inches of nitrous air.

The calces of copper are also soluble in this acid.

*Copper in marine acid.*

100 grs. of copper require 1190 grs. of real marine acid to dissolve them, and also the assistance of a moderate heat, the proportion of acid to that of water being as 1 to  $4\frac{1}{2}$ , that is, its specific gravity being 1,186; if a greater heat be used, more of the acid will be requisite, as much will be dissipated. If the acid be more concentrated, it will act more vigorously.

In my last paper I mentioned, that 8 $\frac{1}{2}$  grs. of copper afforded 86 of marine air: however, I must now add a circumstance which I then did not attend to, which is, that the mercury over which that air was received was acted upon, so that part of the air was due to this action, which invalidates the conclusion I there drew concerning the quantity of phlogiston in marine air, which hence appears to be greater than I there estimated it.

The calces of copper are also soluble in this acid, though not so easily as in the nitrous acid.

*Tin in the vitriolic acid.*

100 grs. of tin require for their perfect solution 872 grs. of real vitriolic acid, whose proportion to water should not be less than as 1 to  $\frac{1}{10}$ , and also the assistance of a strong heat; when the action of the acid has ceased, some hot water should be added to the turbid solution, and the whole again heated. This solution affords 70 cubic inches of inflammable air. Tin is also soluble in a more dilute acid, but not in so great quantity.

The calces of tin (except that precipitated from marine acid by fixed alkalies) are insoluble in this acid.

*Tin*

*Tin in the nitrous acid.*

100 grs. of tin require, for their perfect solution, 1200 grs. of real nitrous acid, whose proportion to water should be at least as 1 to 25, and the heat not exceeding  $60^{\circ}$ : the quantity of air afforded by such solution is only ten cubic inches, and it is not nitrous. The solution is not permanent; for in a few days it deposits a whitish calx, and if the weather be warm bursts the phial. The calces of tin are insoluble in this acid.

*Tin in the marine acid.*

100 grs. of tin require for their solution 413 of real marine acid, whose proportion to water is as 1 to  $4\frac{1}{2}$ , and also the assistance of a moderate heat. This solution affords about 90 cubic inches of inflammable air and 10 of marine air. The calces of tin are nearly insoluble in this acid.

*Lead in the vitriolic acid.*

100 grs. of lead require for their solution 600 grs. of real acid, whose proportion to water is not less than that of 1 to  $\frac{7}{10}$ , and better if the quantity of water be still less; and hence, as with regard to copper, a greater quantity of lead should be employed than is expected to be dissolved. A strong heat is also requisite, and hot water should be added to the calcined mass, though sparingly, as it occasions some precipitation.

This metal is also soluble, but in a very small degree, in dilute vitriolic acid; for it effervesces with spirit of vitriol whose specific gravity is only 1,275.

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The calces of lead are something more soluble in this acid. 100 grs. of vitriol of lead, formed by precipitation, contains 73 of lead, 17 of real acid, and 10 of water. Vitriol of lead, formed by direct solution, contains a large proportion of acid.

*Lead in the nitrous acid.*

100 grs. of lead require for their solution about 78 grs. of real acid, whose proportion to that of water may be as 1 to 11 or 12, and the assistance of heat towards the end. This solution affords but eight cubic inches of nitrous air. The calces of lead are also soluble in this acid; but if much dephlogistified they become less soluble.

100 grs. of minium require 81 grs. of real acid.

100 grs. of nitrous salt of lead contain about 60 of lead.

*Lead in the marine acid.*

100 grs. of lead require 600 grs. of real acid to dissolve them, when the specific gravity of the spirit of salt is 1,141, and also the assistance of heat, by which much of the acid is dissipated. A stronger acid would dissolve more.

The calces of lead are more soluble in this acid than genuine lead. 100 grs. minium require 327 grs. of real acid; but white lead is much less soluble.

100 grs. of horn lead, formed by precipitation, contain 72 of lead, 18 of marine acid, and 10 of water.

*Silver in the vitriolic acid.*

100 grs. of pure silver require to dissolve them 530 grs. of real vitriolic acid, whose proportion to water is not less than that of 1 to  $\frac{1}{10}$ , and when such a concentrated acid is used, it acts slightly even in the temperature of 60°; but for a copious solution

solution a moderate heat is requisite. This solution affords 30 cubic inches of vitriolic air. Standard silver affords more air and requires more acid for its solution. The calces of silver (that is, silver precipitated from its solution in nitrous acid by fixed alkalies, and well-washed, but which still retains some nitrous acid), are soluble even in dilute vitriolic acid, without the assistance of heat. 100 grs. of vitriol of silver, formed by precipitation, contain 74 grs. of silver, about 17 of real acid, and 9 of water.

*Silver in the nitrous acid.*

100 grs. of the purest silver require for their solution 36 of mere nitrous acid, diluted with water in the proportion of one part real acid to 6 of water, applying heat only when the solution is almost saturate. If spirit of nitre be much more or much less dilute, it will not act without the assistance of heat. The last portions of silver, thus taken up, afford no air. *Standard* silver requires about 38 grs. of real acid to dissolve the same proportion of it. And the solution of it affords 20 cubic inches of nitrous air, whereas 100 grs. of silver, revived from *luna cornua*, afford about 14.

*Silver in the marine acid.*

I have not been able to dissolve silver, in its metallic state, in spirit of salt, yet I believe it may be effected, if sufficient time be allowed, as Mr. BAYEN, in his Treatise on Tin, p. 201. says, he dissolved  $3\frac{1}{2}$  grs. of silver by digesting it for some days in two ounces of strong spirit of salt. Leaf silver is also said to be corroded by strong spirit of salt, 1 NEWM. 70. The dephlogisticated marine acid also dissolves it, according to the observations of Mess. SCHEELE and BERGMAN: and so does the phlogisticated

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cated in a vaporous state. 100 grs. of horn silver contain 75  
of silver, nearly 18 of acid, and 7 of water.

*Gold in aqua regia.*

I made several experiments with aqua regia, in which the  
nitrous and marine acids were mixed in different proportions,  
and found *that* to succeed best, in which the quantity of real  
marine acid was to that of the nitrous as 3 to 1, and both as  
concentrated as possible; though if both be very concentrated,  
it is hard to mix them so as to prevent a great quantity from  
escaping, as they effervesce very violently some time after mix-  
ture. 100 grs. of gold require 246 grs. real acid for their solu-  
tion, the two acids being in the above mentioned proportion.

The specific gravity of the nitrous acid I used was 1,465, and  
that of the marine 1,178. The solution is better promoted by  
allowing it sufficient time than by applying heat. The heat I  
used did not exceed 90 or 100°. Very little air is produced, and  
the solution is very slow. Aqua regia made with common salt  
or sal ammoniac and spirit of nitre is much less aqueous, than  
that resulting from an immediate combination of both acids;  
and hence is the fittest for the production of crystals of gold.

Gold is also soluble in the dephlogisticated marine acid, but  
in very small quantity, unless this acid be in a vaporous state,  
for in a liquid state it is too aqueous. In vitriolic and nitrous  
acids it is also insoluble; but the calces of gold are easily solu-  
ble in the marine acid, very slightly in the nitrous, and scarce  
at all in the vitriolic. Gold in its metallic state may be diffused  
through, but not dissolved, by the concentrated nitrous acid.

*Mercury in vitriolic acid.*

100 grs. of quicksilver require for their solution 230 grs. of real vitriolic acid, whose proportion to that of water is at least as 1 to  $\frac{1}{10}$ , and also a strong heat. The air produced is vitriolic. Precipitate, *per se*, is still less soluble.

100 grs. of vitriol of mercury, produced by precipitation, contain 77 of mercury, 19 of acid, and 4 of water.

*Mercury in nitrous acid.*

100 grs. of mercury are dissolved by 28 grs. of real nitrous acid, whose proportion to that of water is as 1 to 1 and  $\frac{1}{1000}$ , and without the assistance of heat. Mercury is also soluble, but in smaller quantity, in a much more dilute acid, with the assistance of heat. The product of air is about 12 cubic inches or less, if heat be not applied. Mr. LAVOISIER found the product of air much greater, which evidently was caused by his using red or yellow spirit of nitre, which already contains much phlogiston. When I dissolved a hundred grs. of mercury in three times more acid than was necessary for its solution, and without heat, I obtained but 7 cubic inches of nitrous air, and the solution was green; but, on applying heat when the solution was over, I obtained 2 more cubic inches, and then the solution was of the colour of oil of olives.

Precipitate, *per se*, is much more difficultly dissolved by nitrous acid than genuine mercury, which I attribute to the attraction of the ærial acid contained in the precipitate.

*Mercury in marine acid.*

The marine acid, in its common phlogisticated state, does not act on mercury, at least in its usual state of concentration;



but Mr. HOMBERG, in the Paris Memoirs for the year 1700, assures us, he dissolved mercury in marine acid, whose specific gravity was 1,300, by keeping it some months in digestion. The authors of the *Cours de Chymie de Dijon* affirm also its solubility in this acid, though in very small quantity. The dephlogistigated marine acid, in a vapourous state, certainly acts upon it, though while in a liquid state it is too weak, by reason of its dilution.

Precipitate, *per se*, is also soluble in marine acid, with the assistance of heat. 100 grs. of sublimate corrosive contain 77 of mercury, 16 of real acid, and 6 of water. 100 grs. of mercurius dulcis contain 86 of mercury, and 14 of acid and water.

*Zinc in vitriolic acid.*

100 grs. of zinc require for their solution 100 grs. of real acid, whose proportion to that of water may be as 1 to 8, 10, or 12, applying heat towards the end, when the acid is almost saturated. A small quantity of black powder always remains undissolved. The product of inflammable air is 100 cubic inches. It is soluble in the concentrated vitriolic acid, with the aid of heat.

100 grs. of vitriol of zinc contain 20 of zinc, 22 of acid, and 58 of water.

The calces of zinc, if not exceedingly dephlogistigated, are also soluble in this acid.

*Zinc in nitrous acid.*

100 grs. of zinc require for their solution 125 grs. of real nitrous acid, whose proportion to that of water is as 1 to 12, applying from time to time a slight heat. If a concentrated acid  
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be used, less will be dissolved, as much of the acid will escape during the effervescence. I could procure no nitrous air from the solution by any management, as the nitrous acid is in part decomposed during the operation.

The calces of zinc, if not too much dephlogisticated, are also soluble in this acid.

*Zinc in Marine Acid.*

The same quantity of zinc requires of this acid 210 grs. the proportion of real acid in the menstruum being as 1 to 9, and using from time to time a slight heat. If a less dilute acid be used, more real acid will be requisite, as much of it will escape during the effervescence.

The calces of zinc are also soluble in this acid.

*Bismuth in vitriolic acid.*

200 grs. of oil of vitriol, whose specific gravity was 1,863, dissolved but three grs. of bismuth in a strong heat; but slightly dephlogisticated a greater quantity. 400 grs. of spirit of vitriol, whose specific gravity was 1,200, dissolved but one grain. The calces of bismuth are much more soluble. The solution of the 3 grs. afforded 4 cubic inches of vitriolic air.

*Bismuth in nitrous acid.*

The solution of 100 grs. of bismuth require but 100 grs. of real nitrous acid, whose proportion to water should be as 1 to 8 or 9. In this last case, a gentle heat may be applied. This solution affords 44 cubic inches of nitrous air. The calces of bismuth are also soluble in this acid.

*Bismuth in marine acid.*

400 grs. of spirit of salt, whose specific gravity is 1,220, dissolved only 3 or 4 grs. of bismuth.

*Nickel in vitriolic acid.*

100 grs. of concentrated vitriolic acid dissolve about 4 of nickel, with the assistance of a strong heat. The calces of nickel are much more soluble.

*Nickel in nitrous acid.*

100 grs. of nickel require for their solution 112 grs. of nitrous acid, whose proportion to water is as 1 to 11 or 12, assisted with a moderate heat. A concentrated acid acts so rapidly that much is dissipated. The product of nitrous air is 79 cubic inches. The calces of nickel are also soluble in this acid.

*Nickel in marine acid.*

200 grs. of spirit of salt, whose specific gravity is 1,220, dissolved 4 or 5 grs. of nickel, without the assistance of heat. A weaker acid dissolves less, and requires the assistance of heat. In all these cases of difficult solution more of the metal will be taken up by distillation and cohobation; but the proportion will be difficult to assign.

The calces of nickel are also difficultly soluble in this acid.

*Cobalt in vitriolic acid.*

100 grs. of cobalt require 450 grs. of real acid, whose proportion to its water is not less than 1 to  $\frac{7}{8}$ , and a heat of

270° at least. By pouring warm water on the dephlogisticated mass a solution is obtained.

The calces of cobalt are still more soluble; even a dilute acid will serve.

*Cobalt in nitrous acid.*

100 grs. of cobalt requires 220 grs. of real nitrous acid, whose proportion to water is as 1 to 4, giving towards the end a heat of 180°.

The calces of cobalt are soluble in this acid.

*Cobalt in marine acid.*

100 grs. of spirit of salt, whose specific gravity is 1,178, dissolves, with the assistance of heat, 2½ grs. of cobalt. A more concentrated acid will dissolve more.

The calces of cobalt are more soluble in this acid.

*Regulus of antimony in vitriolic acid.*

100 grs. of regulus of antimony require for their solution 725 grs. of real acid, whose proportion to water is as 1 to 7½, and a heat of 400°. More regulus should be employed than is expected to be dissolved, and the resulting salt requires a large quantity of water to dissolve it; for the concentrated acid lets fall much when water is added to it. A less concentrated acid will also dissolve this semi-metal, but in smaller quantity.

The calces of antimony, even diaphoretic antimony, are something more soluble.

*Regulus of antimony in nitrous acid.*

100 grs. of this semi-metal require 900 grs. of real nitrous acid, whose proportion to water is as 1 to 12, aided with a heat

30 *Mr. KIRWAN'S Experiments and Observations on*  
heat of  $110^{\circ}$ . The solution, however, becomes turbid in a few days.

The calces of antimony are soluble in a much less degree.

*Regulus of antimony in marine acid.*

100 grs. of spirit of salt, whose specific gravity is 1,220, dissolve about 1 gr. of regulus, with the assistance of a slight heat. Spirit of salt, whose specific gravity is 1,178, also acts upon it, but dissolves still less. I believe the concentrated acid would, in a long time, and with the help of a gentle heat, dissolve much more of it.

The calces of antimony are much more soluble in this acid.

*Regulus of arsenic in vitriolic acid.*

200 grs. of oil of vitriol, whose specific gravity is 1,871, dissolve 18 of regulus of arsenic in a heat of  $250^{\circ}$ . Of these about 7 crystalize on cooling, and are soluble in a large quantity of water.

The calces of arsenic are more soluble in this acid.

*Regulus of arsenic in nitrous acid.*

100 grs. of this semi-metal require 140 grs. of real nitrous acid, whose proportion to water is as 1 to 11, and the assistance of heat. It is soluble in a less or more concentrated acid, but in a lesser degree. This solution affords 102 cubic inches of nitrous air. The barometer at 30, and the thermometer at 60.

The calces of arsenic are also soluble in this acid.

*Regulus of arsenic in marine acid.*

100 grs. of spirit of salt, whose specific gravity is 1,220, dissolve  $1\frac{1}{2}$  grs. of regulus of arsenic; the marine acid, in its common

common dilute state, that is, whose specific gravity is under 1,17, does not at all affect it.

The calces of arsenic are less soluble in this acid than in the vitriolic or nitrous.

We have now gone through most of the bases to which acids are capable of uniting (manganese and platina I have purposely omitted, as I was not possessed of a sufficient quantity of either in that degree of purity requisite for exact experiments). We have also seen the quantity of the mineral acids requisite to saturate each basis, except the metallic bases, all of which require an excess of acid, not only to dissolve them, as in most cases much of it flies off with the phlogiston in an aerial form, but also to keep them in solution. The quantity of any basis, taken up by a given quantity of any of these acids, is easily found; for if 100 grs. of any basis take up, at the point of saturation, or require for their solution, the quantity  $a$  of any acid, the quantity taken up or dissolved by 100 grs. of that acid will be  $\frac{10000}{a}$ .

The proportion of ingredients which I have assigned to different neutral salts appears, at first sight, very different from that which Mr. BERGMAN has ascribed to them. This for some time made me very uneasy, as I have the highest confidence in the skill and judgement of that excellent chymist; but on a strict examination I have found, that the difference is more apparent than real. Mr. BERGMAN has never attempted to ascertain the quantity of *real* acid in any substance; but has, according to the custom of all preceding writers, bestowed the title of *acid* on those liquids which contain it in the most concentrated, or at least in a very concentrated state, but which  
still

still confessedly contain some undetermined proportion of water, and by the quantity of water he commonly understands that which is retained by crystallization: thus, in his first vol. p. 137. he says, that 100 grs. of vitriol of iron contain 23 of iron, 39 of vitriolic acid, and 38 of water. But in his treatise *De Productis Vulcanicis*, § 12. he says, that 100 grs. of vitriol of iron contain 24 of iron, 24 of *dephlegmated* vitriolic acid, and 52 of water; and this last calculation scarcely differs from mine, as I assign to 100 grs. of vitriol 25 of iron, 20 of real vitriolic acid, and 55 of water. The difference manifestly arises from the quantity of water still contained in his dephlegmated acid. The most material difference between us regards the quantity of the mineral acids taken up by alkalies; for, according to his and Mr. SCHEFFER's experiments, they take up more of the vitriolic than of the nitrous, and more of the nitrous than of the marine; whereas, according to Mr. HOMBERG's, Dr. PLUMMER's, Mr. WENZEL's, and my experiments, this does not happen. This difference arises in all probability from the different degrees of evaporation by which the crystals of these salts are obtained; for which reason I did not examine the quantity of the crystals, which must be variable, but that of dry salt, left after thorough evaporation. With regard to the quantity of earth and metallic basis in different salts, Mr. BERGMAN's experiments and mine agree almost intirely.

The advantages resulting from these inquiries are very considerable, not only in promoting chymical science, which, being a physical analysis of bodies, essentially requires an exact determination, as well of the quantity and proportion, as of the quality of the constituent parts of bodies, but also in the practical way. Thus, in the first place, it is well known, that  
several

Several important processes are very inaccurately described by ancient chymical writers, and even by some of a modern date, they frequently, for instance, describe the acid they employed by reference to the quantity of fixed alkali, earth, or metal, a given quantity of such acid was capable of neutralizing or dissolving. Now the foregoing observations immediately inform us of the quantity of real acid capable of performing that effect; the remainder, therefore, must have been water; and the quantity of real acid and water being known, the specific gravity is easily found by the help of the foregoing tables, and thus an acid of the same strength may be formed. Thus SCHLUTTER, in the best treatise on Essaying yet extant\*, informs us, that the best aqua fortis for parting silver from gold is that of which a pound dissolves one mark, that is, half a pound of silver: then 1000 grs. of it should dissolve 500 of silver. Now, by the foregoing experiments, we find, that 100 grs. of alloyed silver require 38 of real acid for their solution; consequently 500 grs. of silver will require 190 grs.; consequently 1000 grs. of such spirit of nitre should contain 190 grs. of real acid and 810 of water. Then recurring to the table of the nitrous acid, I find, by the rule of proportion, that the specific gravity of this acid must be about 1,261; for as 190 is to 810, so is 393 acid to 1675 of water. This proportion of water is somewhat greater than that I used, but SCHLUTTER uses a sand heat.

2dly, The importance of this knowledge in the art of pharmacy is very obvious, especially with regard to medicines formed of metallic substances, whose powers depend on the proportion of their ingredients, and their action on each other.

3dly, This degree of precision must tend considerably to the

\* Vol. I. p. 332. French edition.



improvement of the arts of dying and enamelling, the processes by which many of their ingredients are procured being at present much too vague. Thus the process at present used for preparing the precipitate of CASSIUS frequently fails, the strength of the acids not being sufficiently ascertained.

4thly, The uses of this knowledge in the examination of mineral waters, and in assaying of ores, have been amply proved in the elaborate treatises which the celebrated BERGMAN has lately given us on these subjects. And I may further add, that the knowledge of the quantity of acid requisite for the solution of different metallic substances may also furnish us with a new criterion for distinguishing them from each other, and the purer from their alloys, and in some cases inform us of the quantity and quality of the alloy: thus, 100 parts pure silver require less of the nitrous acid to dissolve them than 100 grs. of standard silver; thus also, by dissolving in spirit of salt any metallic substance sufficiently soluble therein, we may know whether it contains the smallest particle of silver, quicksilver, or arsenic, as these are almost insoluble therein, or of regulus of antimony, cobalt, nickel, or wismuth, of which it also takes but a small proportion.

But the end which of late I had principally in view, was to ascertain and measure the degrees of affinity or attraction that subsist betwixt the mineral acids, and the various bases with which they may be combined, a subject of the greatest importance, as it is upon this foundation that chymistry, considered as a science, must finally rest; and though much has been already done, and many general observations laid down on this head, yet so many exceptions have occurred even to such of these observations as seemed to have been most firmly established, that not only a variety of tables of affinity have been formed,

but many very eminent chymists have been induced to doubt, whether any general law whatsoever could be traced. But, as the judicious BERGMAN well observes, it were much more reasonable to examine the circumstances of these exceptions, which undoubtedly arise from the introduction of new powers, and lay down rules qualified with such restrictions as are observed in the action of these antagonist powers. This is the plan I have followed; but before I proceed to explain myself, I must open the subject in a more general way.

Chymical affinity or attraction is that power by which the invisible particles of different bodies intermix and unite with each other so intimately as to be inseparable by mere mechanical means. In this respect it differs from magnetic and electrical attraction. It also differs from attraction of cohesion in this, that the latter takes place betwixt particles of almost all sorts of bodies whose surfaces are brought into immediate contact with each other; for chymical attraction does not act with that degree of indifference, but causes a body already united to another to quit that other and unite with a third, and hence it is called *elective* attraction. Hence attraction of cohesion often takes place betwixt bodies that have no chymical attraction to each other; thus regulus of cobalt and wismuth have no chymical attraction to each other, for they will not unite in fusion, yet they cohere with each other so strongly, that they can be separated only by a stroke of a hammer.

Hence bodies, which refuse to unite to each other chymically when they are most minutely divided, as when both are in a vaporous or ærial state, or when both are in a liquid state, may be judged, in the first case, to have none; or in the second case, to have at best but a very small affinity to each other. But those that unite, when one of them only is in a liquid state,

may be said to have a strong affinity to each other, and it is thus that acids unite to alkalies, earths, and metals, for the most part.

In order to determine the degrees of affinity, Mr. GEOFFROY has laid it down as a general rule, that when two substances are united, and either quits the other to unite to a third, that which thus unites to this third substance must be said to have a greater affinity to it than to the substance it has quitted. This undoubtedly is the case when only two attractive powers are concerned; thus, when selenite is decomposed by a caustic fixed alkali, it is evident, that the vitriolic acid has a stronger affinity to the alkali than to the earth; but in many cases a decomposition, seemingly single, is in fact double, and the result of the action of more than two powers, and then it is not easy to know which is the greatest, nor consequently to determine the degree of attraction; for instance, the vitriolic acid unites to a mild fixed alkali, and expels the fixed air from it, yet it does not necessarily follow, that the vitriolic acid attracts, or is attracted, by the alkali more strongly than the ærial acid; for though there appears here only a single decomposition, yet in reality a sort of double decomposition takes place, the vitriolic acid giving out its fire to the ærial, while the ærial resigns the alkali to the vitriolic; and hence a decomposition might well take place, even on the supposition that the affinity of both acids to the alkali was equal: therefore, to attain any certainty in this matter, it is necessary to ascertain the quantity and force of each of the attractive powers, and denote it by numbers.

Mr. MORVEAU was the first who perceived the necessity of this calculation, and he has accordingly communicated to us, in numbers, a table of the attractive power of mercury with respect to metals; but his method is incapable of being generalized.

fized. Mr. WENZEL had also an eye to such calculation; but his method is much more defective. It is only this, “To discover (says he) the quantity of affinity which the nitrous acid bears to the different substances with which it is capable of uniting, let small equal cylinders of each of the metals be covered over, except at one end, with melted amber, and then exposed to equal quantities of the same spirit of nitre, and in the same temperature; then let the times of the solution of each be noted. *The affinity of the acid to each of these metals will be inversely as the times necessary for the solution of equal quantities of them.*” And as he well knew that spirit of nitre, of the same degree of concentration, would not act equally on each of them, he required that it should be diluted in some cases, and undiluted in others, and allowance to be made for this in the subsequent calculation. But alkalis and earths are here intirely omitted; and even as to metals no conclusion can be drawn by this method. Tin and regulus of antimony are most rapidly attacked by this acid, lead and copper much more slowly; yet it is well known, that its affinity to lead is much stronger than its affinity to tin, and its affinity to copper greater than to regulus of antimony. Silver and quicksilver are more slowly dissolved, and yet the affinity of the nitrous acid to these metals, as will be seen in the sequel, is by far the greatest.

Neither can this method be in any wise applied to the estimation of the affinities of the other mineral acids; for though the vitriolic and marine acids dissolve very slowly, difficultly, and sparingly, several metals that are copiously and readily dissolved by the nitrous, yet they both have a stronger affinity to those very metals than the nitrous has to them, as is evident with regard to silver, mercury, and lead, which are precipitated from the  
nitrous

nitrous by the vitriolic and marine, though the two first are insoluble in the marine, and all three difficultly soluble in the vitriolic. Accordingly, we do not find that Mr. WENZEL has ever made the proposed experiments, at least he makes no mention of their result.

The discovery of the quantity of real acid in each of the mineral acid liquors, and the proportion of real acid, taken up by a given quantity of each basis at the point of saturation, led me unexpectedly to what seems to me the true method of investigating the quantity of attraction which each acid bears to the several bases to which it is capable of uniting; for it was impossible not to perceive,

First, That *the quantity of real acid, necessary to saturate a given weight of each basis, is inversely as the affinity of each basis to such acid.*

Secondly, That *the quantity of each basis, requisite to saturate a given quantity of each acid, is directly as the affinity of such acid to each basis.*

Thus 100 grs. of each of the acids require for their saturation a greater quantity of fixed alkali than of calcareous earths, more of this earth than of volatile alkali, more of this alkali than of magnesia, and more of magnesia than of earth of allum, as may be seen in the following table.

*Quantity of basis taken up by 100 grs. of each of the mineral acids.*

	Veg. fixed alkali. Grs.	Min. alkali. Grs.	Calcar. earth. Grs.	Vol. alkali. Grs.	Mag- nesia. Grs.	Earth of allum. Grs.
Vitriolic acid	215	165	110	90	80	75
Nitrous acid	215	165	96	87	75	65
Marine acid	215	158	89	79	71	55

As

As these numbers agree with what common experience teaches us concerning the affinity of these acids with their respective bases, they may be considered as adequate expressions of the quantity of that affinity, and I shall in future use them as such. Thus the affinity of the vitriolic acid to fixed vegetable alkali, that is, the force with which they unite, or tend to unite, to each other, is to the affinity with which that same acid unites to calcareous earth as 215 grs. to 110; and to that which the nitrous acid bears to calcareous earth as 215 grs. to 96, &c. But before I proceed further in the comparison of these forces, it is necessary to say something of the nature of *saturation*.

A body is said to be *saturated* with another, when it is so intimately combined with that other as to lose some peculiar characteristic property, which it possesses when free from that other. Thus acids possess the property of changing the juice of turnsol, or infusion of litmus, *red*. According to Mr. BERGMAN one grain of the most concentrated oil of vitriol will give a visible redness to 172,300 grs. of this infusion, and one cubic inch of water, saturated with fixed air (the weakest of all acids, as is generally thought) of which water takes up only about its own bulk, and consequently 253 grs. take up only about half a grain, reddens 50 cubic inches, that is, about 12,650 grs. of the infusion. When acids lose this property they are said to be saturated: and if both bodies are saturated, the compound is said to be *neutralized*.

If an acid be united to less of any basis than is requisite for its saturation, its affinity to the deficient part of its basis is as the ratio which that deficient part bears to the whole of what the acid can saturate. Thus if 100 grs. of vitriolic acid be united to 55 parts only of calcareous earth, its affinity to the deficient

55 parts,

55 parts should be estimated half of its whole affinity, as 55 is the half of 110; but its affinity to the retained part is as its whole affinity.

I shall now shew how all decompositions, in which these three acids and the above mentioned bases are alone concerned, may easily be explained.

In all decompositions we must consider, first, the powers which resist any decomposition, and tend to keep the bodies in their present state; and, secondly, the powers which tend to effect a decomposition and a new union. The first I shall call *quiescent* affinities, and the second sort *divellent*.

A decomposition will always take place when the sum of the *divellent* affinities is greater than that of the *quiescent*; and, on the contrary, no decomposition will happen when the sum of the *quiescent* affinities is superior to, or equal to, that of the *divellent*: all we have to do, therefore, is to compare the sums of each of these powers. Thus, if the solutions of tartar vitriolate and nitrous selenite be mixed, a double decomposition will take place, a true selenite and nitre being the result of such mixture.

Quiescent affinities.		Divellent affinities.	
Vitriolic acid to fixed veget. alkali	215	Vitriolic acid to calcareous earth	110
Nitrous acid to calcareous earth	96	Nitrous Acid to vegetable alkali	215
Sum of the quiescent affinities	311	Sum of the divellent	325

Hence a double decomposition must necessarily happen.

The same double decomposition will be produced if, instead of tartar vitriolate, GLAUBER'S salt be used, *MARGR.* 392.; for the sum of the quiescent affinities is 261, and that of the *divellent* 275. So also, if *vitriolic ammon.* be used for the sum of

of the quiescent is 186, and that of the divellent 195, or Epsom salt, 1 MARGR. 390. Mem. Par. 1778, p. 339. or *allum*, 1 MARGR. 387. The determinations, however, with regard to allum are not quite so exact as the foregoing; because allum, whether vitriolic, nitrous, or marine, constantly retains an excess of acid, the exact point of saturation cannot be found as I have already remarked, and is well known: however, the superiority is on the side of the divellent affinities, as it should be. If, instead of a solution of nitrous selenite, that of marine selenite be mixed with the solutions of the above mentioned vitriolico-neutral salts, the same sort of double decomposition will happen, and a true selenite will be formed, 1 MARGR. 382; and on calculation it will be constantly found, that the sum of the divellent constantly exceeds that of the quiescent affinities.

So also, if a solution of tartar vitriolate be mixed with a solution of nitrous or marine Epsom, a double decomposition will take place, though no visible change will appear in the mixed liquor, as vitriolic Epsom is exceeding soluble in water, and therefore is not precipitated as selenite is, on account of its insolubility, Mem. Par. 1778, p. 338. In the first case, the sum of the quiescent powers is 290, and of the divellent 295; in the second case, that of the quiescent is 286, and of the divellent 295.

If a solution of GLAUBER's salt be mixed with that of nitrous or marine Epsom, an invisible double decomposition will also happen, Mem. Par. *ibid.* Hence Mr. QUATREMERE DIJON-VAL, who lately denied this double decomposition (ROZ. Mai 1782, p. 392.) was certainly deceived. In the first case, the sum of the quiescent affinities is 240, and of the divellent 245; in the second case, that of the quiescent is 236, and of



the divellent 238. Further, if a solution of nitre be mixed with a solution of marine selenite, an invisible double decomposition will ensue, *Mem. Par.* 1778, p. 341.; the sum of the quiescent powers being 304, and of the divellent 313.

If a solution of nitrous Epsom be mixed with that of marine selenite, a double decomposition will be the consequence, 17 ROZ. 393.: the sum of the quiescent affinities being 164, and that of the divellent 167.

From all which I collect, first, that the quantity of each affinity, as here determined, perfectly coinciding with all the facts hitherto known, which are pretty numerous, may be looked upon as exact or nearly so. 2dly, That these decompositions are perfectly consistent with the superior affinity which hitherto has been generally ascribed to the vitriolic and nitrous acids with fixed alkalies over that which these acids bear to earths, and do not in the least infringe the received laws of affinities, as *Mess. MARHER, MONNET, and lately Mr. CORNETTE*, in the *Memoirs of Paris* for 1778, p. 339. do insinuate.

There is a fact, however, in that valuable repository of chymical knowledge, *Mr. CRELL's Chymical Journal* \*, which at first sight seems contrary to one of the above determinations; it is there said, that if solutions of one part allum and two parts common salt be mixed together, evaporated to a certain degree, and set to crystallize, a *GLAUBER's salt* will be found; yet in this case the sum of the quiescent affinities is 233, and that of the divellent but 223. I repeated this experiment without success, and indeed the author owns it never succeeds but during the most intense cold.

If it does succeed at all, the decomposition must arise from a large excess of acid in the allum, which acted upon and de-

\* 6 *TREIL*. p. 78.

composed

composed the common salt; and this explanation is confirmed by the small proportion of GLAUBER's salt, which is said to be obtained by this process; for from 30 lbs. of common salt and 16 lbs. of allum only, 15 lbs. of GLAUBER's salt were produced; whereas, if the whole of the allum were decomposed, there should be formed, according to my computation of the proportion of acid in different salts,  $29\frac{1}{2}$  lbs., and, according to Mr. BERGMAN's, 22 lbs. of GLAUBER's salt.

Besides these powers there exists another which neutral salts possess, of uniting to certain substances, without suffering any, or but a very small, decomposition; and thus forming *triple salts*, and sometimes quadruple. This often causes anomalies, and has not as yet been sufficiently investigated \*. Volatile alkalies in particular possess this power; and hence, perhaps, arises the difference between Mr. BERGMAN's table and mine, with regard to them and magnesia; for though, when perfectly caustic, they do not perfectly precipitate magnesia from Epsom salt, it is because they combine with this salt and form a triple salt.

According to my table, the three mineral acids have the same affinity to vegetable fixed alkalies, which will undoubtedly appear extraordinary to many, as it is well known, that the vitriolic acid decomposes both nitre and salt of sylvius; but it should be remarked, that tartar vitriolate is also decomposed by the nitrous and marine acids, as Mr. BAUME, MARGRAAF, and BERGMAN, have found; and nitre is decomposed by the marine acid, as Mr. CORNETTE has shewn at large in the Memoirs of Paris for 1778; and not only these salts, but also GLAUBER's salt and vitriolic ammoniac, are decomposed by the

\* In my next paper I shall examine some exceptions arising from this source.

nitrous acid; and also these salts, together with cubic nitre and nitrous ammoniac, are decomposed by the marine acid, as Mr. BERGMAN and Mr. CORNETTE have remarked: all which shew, that these decompositions are the effect of a double affinity, or at least of compound forces. I always suspected they arose from the different capacities of these acids for elementary fire; but as the subject appeared to me of importance, for greater certainty I made a series of experiments which differ from those hitherto made in several respects, particularly in this, that no heat was applied, and the decompositions were discovered, not by crystallization, but by tests.

First, I procured equal weights of each of the mineral acids, containing each the same quantity of real acid; and throwing each suddenly on an ounce of the same oil of tartar, I had the following results, the temperature of all, before mixture, being  $68^{\circ}$  of FAHRENHEIT. 100 grs. of vitriolic acid, containing 26,6 grs. of real acid projected on 480 of oil of tartar, raised the thermometer to  $138^{\circ}$ .

100 grs. of spirit of nitre, which also contained 26,6 grs. real acid, projected in the same manner on 480 grs. of the same oil of tartar, produced a heat of  $120^{\circ}$ .

100 grs. of spirit of salt, whose specific gravity was 1,220, and which contained 26,6 grs. of real acid, projected on the above quantity of the same oil of tartar, raised the thermometer from  $69$  to  $129^{\circ}$ .

Hence it follows, that the vitriolic acid contains more specific fire, or at least gives out more on uniting to fixed alkalies, than either the nitrous or marine; and, therefore, when the vitriolic acid comes in contact with either nitre or salt of sylvius, its fire passes into these acids, which are thereby rarefied to a great degree, and are thus expelled from their alkaline basis,

basis, which is then seized on by the vitriolic. This explanation is confirmed by the following experiments.

Into 400 grs. of spirit of vitriol, whose specific gravity was 1,362, I put 60 grs. of nitre. The thermometer fell from 68 to 60, and during this time the nitrous acid was not expelled, for I put in some filings of copper, and they were not in the least acted upon; but in five minutes after, they visibly effervesced, which shews that the nitrous acid began to be expelled.

Again, to 400 grs of oil of vitriol, whose specific gravity was 1,870, I put 60 grs. of nitre: the thermometer immediately rose from 68 to 105°, and the nitrous acid was expelled in the form of a visible fume. These experiments prove, first, that neutral salts are not decomposed, by mere solution, in an acid different from that which they possess. 2dly, That the nitrous acid, being converted into vapour, had imbibed a large quantity of fire. But as the vitriolic, in both these experiments, was in much larger quantity than was necessary to saturate the alkaline basis of the nitre, I put 60 grs. of nitre into 64 of the above spirit of vitriol, which contained the same quantity of real vitriolic acid as the 60 grs. of nitre did of the nitrous, and added 40 grs. of water, and also a few grains of filings of copper. In less than two hours the copper was acted upon, and consequently the nitrous acid was expelled.

Again to about 400 grs. of oil of vitriol, whose specific gravity was 1,870, I put 100 grs. of common salt; it immediately effervesced, and gave out the marine acid in the form of a white vapour. A thermometer held in the liquor rose but 4°; but when placed in the froth it rose to 10°, and fell again on being put into the liquor: whence it follows, that the vitriolic acid gave out its fire to the marine, and that this latter received  
more

more than it could absorb even in the state of vapour, and hence communicated heat to the contiguous liquor.

From these experiments it is evident, that the nitrous and marine acids receive fire from the vitriolic, and are thrown into a vaporous state, or at least so much rarefied as to be expelled from their alkaline basis, notwithstanding that their affinity to that basis may be equally strong with that of the vitriolic.

I next proceeded to examine how tartar vitriolate and GLAUBER's salt are decomposed by the nitrous acid. Into 400 grs. of spirit of nitre, whose specific gravity was 1,355, and which contained about 105 grs. of real acid, I put 60 of pulverised tartar vitriolate. The thermometer, which stood at 68°, was not in the least affected by standing in this mixture, and there was scarce any sign of solution. To try whether the vitriolic acid was disengaged, I threw into the liquor a few grains of powdered regulus of antimony: in 24 hours the vitriolic acid was in part disengaged, for the regulus was acted upon, and the liquor became greenish. This semi-metal being soluble in a mixture of the vitriolic and nitrous acids, but in neither singly, however, a great part of the tartar vitriolate still remained undissolved. Afterwards I put the same quantity of tartar vitriolate into 400 grs. of spirit of nitre, whose specific gravity was 1,478; the thermometer rose from 67 to 79°, the tartar vitriolate was quickly dissolved, and the regulus of antimony shewed the vitriolic acid was disengaged.

Hence it follows, that in the last experiment the nitrous acid having the same affinity to the alkaline basis as the vitriolic, but giving out, during the solution, more fire than was necessary to perform the solution, the vitriolic receiving this fire was disengaged; for as it cannot unite to alkalis without giving out fire, so when it receives back that fire it must quit them. The reason

reason why the nitrous acid, which specifically contains less fire than the vitriolic, gives out so much, is, that its quantity in both these experiments is far greater than that of the vitriolic, it being in the first as 105 to 17; and in the second as 158 to 17.

For this reason, to 60 grs. of spirit of nitre, whose specific gravity was 1,355, I added 1000 grs. of water, and into this dilute acid I put 60 grs. of tartar vitriolate, which contained exactly the same quantity of acid as the 60 grs. of spirit of nitre. After eight days the tartar vitriolate was almost intirely dissolved, yet I could perceive no sign of its decomposition, and after evaporation no nitre was found. Hence I conclude, that the nitrous acid can never decompose tartar vitriolate without the assistance of heat, but when its quantity is so great that it contains considerably more fire, and by the act of solution is determined to give out this fire. The decomposition of GLAUBER's salt and vitriolic ammon. (neither of which, as Mr. BERGMAN observed, is ever total) may be explained in the same manner; whereas the vitriolic, ever so dilute, decomposes both nitre and nitrous ammoniac totally. Tartar vitriolate is also decomposed by the marine acid, though very slowly, for the same reason, and in the same circumstances, as it is decomposed by the nitrous acid, as appears by the following experiments. Into 400 grs. of spirit of salt, whose specific gravity was 1,220, I put 60 grs. of tartar vitriolate. The thermometer was not in the least affected, and the salt dissolved very slowly. To try whether the vitriolic acid was disengaged, I added some pulverised bismuth; in twelve hours part of the bismuth was dissolved, and could not be precipitated by the affusion of water, a sign that it was held in solution by the compound acid, which alone hath the property of preventing

preventing its precipitation by the affusion of water, as Mr. WENZEL has discovered. Here the quantity of marine acid was much greater than that of the vitriolic, and consequently it contained more fire; but this circumstance alone is not sufficient, it must besides be determined to give out that fire by the act of solution. This appears by the experiments of Mr. CORNETTE; for when he mixed half an ounce of tartar vitriolate, *previously dissolved* in water with two ounces of spirit of salt, the tartar vitriolate was not decomposed, *Memoirs, Paris, 1778, p. 49.*; for it being already dissolved, no cold or heat was generated by mixing it with the spirit of salt, and consequently the latter did not give out any fire. Mr. CORNETTE also observed, that GLAUBER's salt is easier decomposed by the marine acid than tartar vitriolate; this I have also experienced, and the reason is, first, because GLAUBER's salt is more easily soluble in spirit of salt than tartar vitriolate; and, secondly, because its alkaline basis takes up a greater quantity of the real marine acid than of the vitriolic, whereas the basis of tartar vitriolate takes up an equal quantity of both acids; consequently the marine gives out more fire in uniting to the basis of GLAUBER's salt than on uniting to that of tartar vitriolate.

Vitriolic ammoniac is also decomposed by the marine acid for the same reason; but in all these cases the quantity of the marine acid must much exceed that of the vitriolic, or no decomposition will take place. The decomposition of nitro-neutral salts by the marine acid depends on the same principles. Mr. CORNETTE found, that cubic nitre was more easily decomposed than prismatic nitre, and accordingly, during the solution of prismatic nitre, only  $3^{\circ}$  of cold were produced; but, during that of cubic nitre, the thermometer fell  $6^{\circ}$ , a sign that the spirit of salt gave out more fire in the latter case than

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in the former, and its quantity must always be greater than that of the nitrous acid contained in the mineral alkaline basis, because this basis requires for its saturation more of the marine than of the nitrous acid, as we have already seen.

Yet the nitrous acid, in its turn, decomposes salt of sylvis and common salt, as Mr. MARGRAF has shewn; but it must always be in greater quantity than the marine, in order to contain a sufficient quantity of fire for that effect. To 400 grs. of colourless spirit of nitre, whose specific gravity was 1,478, I put 60 grs. of common salt, it quickly effervesced and grew red; yet the thermometer rose but 2°, a sign that the marine acid had absorbed the greater part of the fire which the nitrous had given out, and was thus expelled: besides, in this case, the superior affinity of the nitrous acid to the mineral basis hastened the decomposition; and hence the decomposition happens without solution, whereas the marine acid does not decompose cubic nitre until it has dissolved it, which is worthy of notice. This mutual expulsion of the nitrous and marine acids by each other is the true reason why aqua regia may be made, as well by adding nitre or nitrous ammoniac to spirit of salt, as by adding common salt or sal ammoniac to spirit of nitre, as Mr. CORNETTE has well remarked.

Selenite is decomposed neither by the nitrous nor by the marine acids, as Mess. CHAPTAL and CORNETTE have observed. The reason is evident on the above principles; it is dissolved by neither without the assistance of heat, and then the solution is performed by a *foreign* heat, and not by that which these acids give out when they act without the assistance of heat.

Lastly, whenever a vitriolico-neutral salt, decomposed by either the nitrous or marine acid, is evaporated to a certain degree, the vitriolic expels these acids in its turn; for the free



part of the former acids being expelled by the heat of evaporation, the neutral salts begin to crystallize, and consequently give out heat ; but the vitriolic being then in greater proportion re-acts on these salts, restores their specific fire to their acid principle, and recombines with their alkaline basis, as already explained.

Hence, though allum is in reality decomposed by the nitrous and marine acids, yet when the solution of it in either of these acids is evaporated to a certain degree, the vitriolic acid, of which it contains a larger proportion than any other terrene salt, re-acts on the nitrous and marine allums, and expels their acids, as Mr. CHAPTAL has shewn.

In explaining these phenomena I have all along supposed the doctrine of Dr. BLACK to be well known, *viz.* that solids absorb heat during their solution. Both the heat and cold, produced in different solutions, seem to me to depend on the same principle. If the menstruum gives out only *so much* of its fire as the solvend can absorb, or *less*, then cold is produced ; but if it gives out *more* of its specific fire than the solvend can absorb, this surplus becomes sensible, and affects the thermometer by producing heat in proportion to its quantity.

*Of the affinity of the mineral acids to metallic substances.*

Having thus, in every instance, established the agreement betwixt the quantity of any alkaline or terrene basis, taken up at the point of saturation by a given weight of any of the three mineral acids, and the quantity of affinity which each of these acids bears to such basis, I naturally extended my views to metallic substances, to try whether this coincidence could be traced with regard to them also ; but the difficulties that occurred in this inquiry were so great, that the same degree of certainty must not be expected as in the foregoing part.

Metallic

Metallic substances, when freed from all foreign mixture, are obtained either in a reguline state, or in that of a calx. These calces, if formed by fire, are constantly combined with more or less of the ærial acid, which is very difficultly extracted from them, and very soon re-absorbed; and if formed by solution, they as constantly retain a portion of their solvent or precipitant, so that the precise weight of the really metallic part is difficultly ascertained. But though this should easily be effected, still they would for the most part be unfit for my purpose; because most of them, when much dephlogisticated, are insoluble in some or all the acids: hence I chose metals in their metallic state for the subject of my experiments. These consist of specifically different earths and phlogiston, and of this they must lose a part before they can be dissolved in acids; but, besides that which escapes in an ærial form, much more of it, though separated from the metallic earth, is yet retained in the solution by the compound of acid and calx. It is this calx, thus differently dephlogisticated by the different acids, whose proportion I endeavoured to ascertain.

The great difficulty that occurred in this inquiry was, that of finding the exact quantity of acid necessary to saturate the metallic substances; for all metallic solutions turn solution of litmus red, and consequently contain an excess of acid. And the reason is, because the salts, formed by a due proportion of metallic calx and acid, are nearly insoluble in liquids that do not contain a further quantity of acid; and in some cases this quantity, and even its *proportion to the aqueous part of the liquor*, must be very considerable, as in solutions of bismuth. Hence I in vain endeavoured, by caustic alkalies and lime-water, to deprive these solutions of this excess; for when deprived even of only part of it, many of the metals precipitated, and all

would, if deprived of the whole of it. On this account I was obliged to use different methods, of which I shall here give an instance. With regard to the solution of silver in the nitrous acid, as it could be had extremely saturated I began with it. 657 grs. of this solution contained, according to my calculation, and allowing for the quantity of acid carried off in the nitrous air, 31,3<sup>1</sup>/<sub>2</sub> grs. of real acid, and 100 grs. of silver. Of this solution I found that 9 grs. gave a visible *red tint* to as much of a dilute solution of litmus, as a quantity of spirit of nitre, which contained  $\frac{1}{10}$ ths of a grain of real acid, and therefore I judged these 9 grs. to contain an excess of acid, amounting to  $\frac{1}{10}$ ths of a grain; and if 9 grs. contain such an excess, then the whole solution must have contained an excess amounting to  $5\frac{1}{10}$ ths of a grain, deducting which from 31,38, we find the quantity of acid saturated by 100 grs. of silver to be 25,78 grs. In this manner I proceeded with most other metallic solutions. The vitriolic solutions of tin, wismuth, regulus of antimony, nickel, and regulus of arsenic, containing a large excess of acid, I saturated part of it with caustic volatile alkali before I tried them with the infusion of litmus, and I used the same expedient with the nitrous solution of iron, lead, tin, and regulus of antimony, and all the marine solutions. The proportion of vitriolic and marine acid taken up by lead, silver, and mercury, I determined by computing the quantity of real acid necessary to precipitate these metals from their solutions in the nitrous acid; and of all the determinations these appeared to me to be the most exact. However, as all the vitriols of these metals are, though in a slight degree, soluble in the nitrous acid, I was obliged to rectify the result from other considerations, and the same necessity occurred with regard to the marine salts of lead and mercury.

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The result of these experiments was, that 100 grs. of each of these acids take up, at the point of saturation of each metallic substance, dephlogisticated to such a degree as is necessary for its solution in each acid, the quantities expressed in the following table, which denote their degree of affinity to each metal.

*Table of the affinity of the three mineral acids to metallic substances.*

100 grs.	Iron.	Copper.	Tin.	Lead.	Silver.	Mercury.	Zinc.	Wismuth.	Nickel.	Cobalt.	Reg. of antimo.	Reg. of arsenic.
Vitriolic acid	270	260	138	412	390	432	318	250 310	320	360	200	260
Nitrous acid	255	255	120	365	375	416	304	290	300	350	194	220
Marine acid	265	265	130	400	420	438	312	250 320	275 310	370	198	290

Yet I cannot say that these numbers are *precisely* such as I could extract from my observations on the colour of the solution of litmus; for these indications are so precarious that I did not absolutely confide in them, but adjusted the numbers, as I thought other phenomena required. However, the deviations were not so considerable as to induce a doubt that metallic earths had not almost all a stronger affinity to the three acids than even fixed alkalies. Nevertheless, the common tables, which postpone metallic substances to all others, are in reality just; they only require a different denomination, being in fact tables of *precipitation* rather than of *affinity*, as far as they relate to metallic substances, expressing by their *order*, what metallic substances precipitate others from the different acids. But these precipitations are constantly the result of a double affinity and decomposition,

decomposition, the *precipitating* metal yielding its phlogiston to the *precipitated* metal, while the *precipitated* metal yields its acid to the *precipitant*. Nor has this escaped the sagacity of Mr. BERGMAN, 2 N. Act. Upf. 205. who has even confirmed it by experiments which I have repeated, and found exact. Thus, though copper, in its metallic form, precipitates silver and mercury from the nitrous acid with great ease, yet the calx of copper will precipitate neither. The superior affinity of acids to metallic earths, in preference to alkalies and unmetallic earths, requiring further proof, I shall here demonstrate it in a few instances, with regard even to those metals which are commonly thought to have the least affinity to acids. And, first, that the nitrous acid has a stronger affinity to silver than to fixed alkalies, appears by a curious experiment of Mr. MONNET's (*Dissolution des Metaux*, p. 159.). If a solution of silver in nitrous acid be poured into a mixed solution of fixed alkali and common salt, the silver will be precipitated by the marine acid of the common salt, and not by the free alkali contained in the liquor, for a *luna cornua* is found. Now if the nitrous acid had a greater affinity to the free alkali than to the silver, it is evident, that the decomposition would be wrought by the free alkali, and then the silver would be precipitated pure, and not in the state of horn silver; but as it is precipitated in the state of horn silver, it is plain, its precipitation was not effected by a single but by a double affinity. From whence it also follows, that the marine acid has a greater affinity to silver than the nitrous has to fixed alkalies. I repeated this experiment with a solution of lead and also of mercury in the nitrous acid, and the result was similar, horn lead and marine salt of mercury being formed.

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With regard to mercury, the experiments of Mr. BAYEN are well known: he has shewn that vitriol of lead and sub. corrosive can be deprived of no more than half of their acid even by caustic fixed alkalies, 3 ROZ. 293.

Again as to lead, if perfectly dry common salt be projected on lead heated to incandescence, the common salt will be decomposed, and horn lead formed, 1 MARGRAF. 35 and 38. Nor can this be attributed to the volatilization of the acid by heat; for the alkali is as fixed as the lead, and must therefore be caused by the greater affinity of the calx of lead, to which, when dephlogisticated, the acid can unite. Mr. SCHEELÉ informs us, that if a solution of common salt be digested with litharge, the common salt will be decomposed, and a caustic alkali produced, SCHEELÉ on Fire, p. 175. He also decomposes common salt by simply letting its solution slowly pass through a funnel filled with powdered litharge. \* Mr. TURNER daily decomposes common salt by means of litharge. Mr. SCHEELÉ also decomposes marine selenite, by means of litharge, through simple mixture, without the assistance of heat, and the calcareous earth is separated in a caustic state; which shews that this salt is decomposed by the single superior affinity of the metallic calx to the marine acid, SCHEELÉ on Fire, p. 174.

That acids have less affinity to volatile alkalies than to several metallic substances appears in sundry instances. Horn silver is soluble in volatile alkalies, as is well known. Now, if this solution be triturated with four times its weight of mercury, the marine acid will combine with the mercury, and not with the volatile alkali; for a *mercurius dulcis*, and not a sal ammoniac, will be formed, as Mr. MARGRAF has shewn, 1 MARGRAF. 286. If two parts sal ammoniac and one of filings of iron be

\* SCHEFFER Chymische Focales, § 59.

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trituated together, the smell of the volatile alkali will immediately be perceived \*; or if, instead of iron, minium, or diaphoretic antimony, or zinc, be used, that smell is perceived as soon as they are mixed, 9 Mem. Scav. Etrang. p. 575. MONNET, Diff. Met. 209. But it will naturally be asked, how then it happens, that all metallic solutions are precipitated by alkalies and earths? The answer is easy; all metallic salts are held in solution by an excess of acid. If alkalies and earths did nothing more than absorb this excess of acid, a precipitation ought to take place; but they do still more, for they take up the greater part even of the proportion of acid necessary to saturate the metallic earth, and this they are enabled to do by means of a double affinity; for during the solution of metals, only a comparatively small part of the phlogiston escapes out of the solution, the remainder is retained by the compound of acid and calx: when, therefore, an alkali or earth is added to such a solution, the phlogiston quits the acid, and re-combines with the calx, while the greater part of the acid unites to the precipitant. Notwithstanding this great affinity of metallic earths to acids, salts, whose basis is a fixed alkali or earth, are in few instances decomposed by metals or their calces, by reason of the inability of the acids while combined with these basis, and thereby deprived of a great part of their specific fire, to volatilize the phlogiston combined with the metallic earths, which must necessarily be expelled before an acid can combine with them. And as to metallic calces, they are generally combined with fixed air, which also must be in part expelled.

But ammoniacal salts, containing much more fire (for they absorb fire during their formation) for that reason act much more powerfully on metals. Allowing then the affinity of the

\* MONNET, Dissol. Met. 72.

mineral acids to metallic substances to be as above, all double decomposition in which only salts, containing these acids united to alkaline terrene or metallic bases, are concerned, admit of an easy explanation; nay, I am bold to say, they cannot otherwise be explained. Thus if a solution of tartar vitriolate, and of silver in the nitrous acid, be mixed in proper proportions (which is always to be understood), nitre and vitriol of silver will be formed, and this latter for the most part precipitated.

Quiescent affinities.			Divellent affinities.		
Nitrous acid to silver	-	375	Nitrous acid to vegetable alkali		215
Vitriolic acid to vegetable alkali		215	Vitriolic acid to silver	-	390
Sum of the quiescent	-	590	Sum of the divellent powers		605

So also if, instead of a solution of tartar vitriolate, that of GLAUBER's salt, or of vitriolic-ammoniac, or selenite, Epsom, or allum, be used; for in all these cases the balance is constantly in favour of the divellent powers, yet the solutions of selenite and allum produce but a slight precipitation.

I also found, that the solution of silver is precipitated by the vitriolic solutions of iron, copper, tin, and probably by many other vitriolic solutions, if for no other reason at least for this, that they constantly contain an excess of acid; but if a saturate solution of silver be mixed with a very saturate solution of lead or mercury in the vitriolic acid, the silver will not be precipitated, as I have observed; and in both cases the balance is in favour of the quiescent affinities.

The nitrous solution of silver is also decomposed, and the silver precipitated by all *marino* neutral salts, whether the basis be alkaline, terrene, or metallic, as I have experienced, and



these decompositions are constantly indicated by the balance of affinities exhibited as above.

In the same manner silver is precipitated from the *vitriolic* acid by *marine* neutral salts, whether their basis be alkaline, terrene, or metallic, as I have found on trial, and as the balance of affinities requires.

The nitrous solution of lead is also decomposed, and the lead for the most part precipitated (unless the solution be very dilute) in the form of vitriol of lead by all the *vitriolico* neutral salts; and also by all the *marine* neutral salts, except marine salt of silver, which only precipitates it by virtue of its excess of acid.

The marine solution of lead is decomposed by all *vitriolico* neutral salts, except selenite and vitriol of nickel, which can only precipitate it by virtue of an excess of acid.

The nitrous solution of mercury is also decomposed, and the mercury for the most precipitated in the form of vitriol of mercury by all *vitriolic* neutral salts, except vitriol of lead, which can only decompose it by an excess of acid.

Nitrous solution of mercury is also decomposed by *marine* neutral salts, except the marine salt of silver and lead, which can only affect it by an excess of acid.

Vitriol of mercury is also decomposed by *marine* neutral salts, which decomposition is also apparent by exposing the antagonist powers; yet a precipitation does not always appear as I have remarked, particularly when marine allum is used, which I attribute to the facility with which a small quantity of the marine salt of mercury is soluble in an excess of acid. Marine salt of silver decomposes vitriol of mercury, only through its excess of acid.

Hence we see why horn silver can never be reduced by fixed alkalies without loss, as Mr. MARGRAAF has shewn, I MARGR. 277.; nor could it be decomposed at all, but that the action of heat helps that of the alkali.

If to a solution of sublimate corrosive oil of vitriol be added, a precipitation will appear; but, as Mr. BERGMAN well remarks, this does not proceed from a decomposition, but from a subtraction of the water necessary to keep the sublimate dissolved.

If to a solution of vitriol of iron some nitrous acid be added, it immediately becomes turbid, because the nitrous acid dephlogisticates the calx of iron too much, but the addition of more acid restores the transparency, as the dephlogisticated calx is still soluble by a greater quantity of acid. I omit a number of other curious phenomena, which are explicable on these principles.

I have assigned in the foregoing table two different affinities to the vitriolic acid with regard to wismuth, and also to the marine acid with regard to nickel and wismuth. The first shews that which these acids bear to those metals, when dephlogisticated only by solution in those acids. The second number, that which the acids bear to them when more dephlogisticated, as they are, when dissolved in the nitrous acid. On the other hand, all the acids have less affinity to the calces of iron, zinc, tin, and antimony, when they are dephlogisticated to a certain degree; but as I could give no criterion of this dephlogistication, I did not attempt to indicate the diminution it causes in the affinities of acids.

*Of the precipitation of metals by each other from the mineral acids.*

I am now come to the last point of my inquiry, and the most difficult to be set forth with that degree of precision which I have been enabled to attain in the former parts; for, in the first place, it is necessary to find the quantity of phlogiston in each of them, not only in general, but according to their various degrees of dephlogistication by each of the acids. In this last particular I cannot assert that I have attained any thing like a certainty, yet I hope what I advance may not be useless to chymical readers, as it is not altogether groundless, as it contradicts no chymical fact, but, on the contrary, is agreeable to many, and affords a ready solution of all the phenomena.

*Of the absolute quantity of phlogiston in metals.*

The proportion of phlogiston in metallic substances relatively to each other has been investigated in so masterly a manner by Mr. BERGMAN, that I lay it down as the ground of my inquiries. After his discovery all that remained was to find the absolute quantity of it in any one metal, for then, by an easy calculation, it may be determined in all the rest. The substance I chose for this purpose was regulus of arsenic, as being most capable of dephlogistication by nitrous acid, though not altogether so.

From 100 grs. of regulus of arsenic, dissolved in dilute nitrous acid, as already seen, 102 cubic inches of nitrous air and  $\frac{1}{4}$ ths are obtained, barometer at 30°, thermometer at 60°. I must add, that I made the experiment on 5 grs. only, so that the calculation relates only to the quantity of air which 100 grs.

grs. *should* give. I repeated the experiment three times with the same success. I attempted getting more air from the residuum left by a gentle evaporation, but though fresh spirit of nitre grew red with it, the quantity of air was quite inconsiderable.

Now this quantity of nitrous air contains 6,86 grs. of phlogiston, according to the calculation to be seen in my former paper; and hence I conclude, that 100 grs. of regulus of arsenic contains 6,86 grs. of phlogiston. This regulus was made by Mr. WOLFE, and perfectly bright.

Hence the relative proportion of phlogiston in metals being, as found by Mr. BERGMAN, and set forth in the first column of the following table, the absolute quantity will be as shewn in the second column.

	Relative quantity of phlogiston.				Absolute quantity.	
100 grs. Gold	-	-	394	-	-	24,82
Copper	-	-	312	-	-	19,65
Cobalt	-	-	270	-	-	17,01
Iron	-	-	233	-	-	14,67
Zinc	-	-	182	-	-	11,46
Nickel	-	-	156	-	-	9,82
Regulus of antimony	120	-	-	-	-	7,56
Tin	-	-	114	-	-	7,18
Regulus of arsenic	109	-	-	-	-	6,86
Silver	-	-	100	-	-	6,30
Mercury	-	-	74	-	-	4,56
Wismuth	-	-	57	-	-	3,59
Lead	-	-	43	-	-	2,70

This point being, as I conceived, of some importance, I endeavoured to ascertain it still further by other experiments: and

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as silver loses a certain quantity of phlogiston, which escapes and separates from it during its solution in nitrous acid; I conceived, that if the solution was exposed to nothing from which it could re-obtain phlogiston, and thus distilled to dryness, and intirely separated from the acid, as much silver should remain unreduced as corresponded with the quantity of phlogiston lost by it. And if this quantity of phlogiston corresponded with that assigned to silver in the foregoing table, that then this table was just.

For this purpose I dissolved 120 grs. of clean filings of standard silver in dilute dephlogisticated nitrous acid, and obtained from it 24 cubic inches of nitrous air. This solution I gently evaporated to dryness; by the evaporation I found a little of the silver volatilized, but not more than a quarter of a grain. I then distilled the dry residuum, and kept it an hour in almost a white heat in a coated green glass retort. During the distillation abundance of the nitrous acid passed off, a green and white sublimate arose in the neck of the retort, and some passed even into the receiver. When all was cold I broke the retort, the inside of which was penetrated into its very substance with a yellow and red tinge, and partly covered over with an exceeding fine silver powder, which could scarcely be scraped off. The remainder of the silver was perfectly white and free from acid, but not melted into a button, and when collected weighed 94 grs.; therefore 26 grs. were lost, that is, were sublimed or vitrified; but of these 26 grs. 9 grs. were copper (for 100 grs. standard silver contain  $7\frac{1}{2}$  of copper); therefore, only 17 grs. of pure silver remained unreduced, being either volatilized or vitrified. The whole quantity of pure silver in 120 grs. of standard silver amounts to 111 grs.; then if 111 grs. of pure silver lose 17 by reason of its  
loss

loss of phlogiston, 100 grs. of pure silver should lose 15,3; and by the above table 15,3 grs. of silver should contain 0,945 of a grain of phlogiston. Let us now see whether this quantity of phlogiston corresponds with that which 100 grs. of pure silver really lose by solution in nitrous acid. 100 grs. of pure silver afford, as already said, 14 cubic inches of nitrous air, which, by my computation, contain 0,938 of a grain of phlogiston, which differs from 0,945 only by  $\frac{7}{1000}$ . The unreduced part of the silver was 15,3 grs.; and, by calculating what it should be by reason of the loss of the phlogiston contained in the nitrous air, it would amount to 14 and  $\frac{2}{3}$ ths of a grain, a difference certainly immaterial.

In this experiment, only as much of the silver sublimed as could not regain phlogiston; the remainder regained it from the nitrous air absorbed by the solution, and also from that which remained united to the acid and calx. If this were not so, I do not see why the whole of the silver would not sublime.

Again: Dr. PRIESTLEY having several times dissolved mercury in the nitrous acid, and revived it by distilling over that acid, constantly found a considerable proportion of it unreduced. To try whether that proportion corresponded with my calculation, I have examined the experiment which he made with most care, and which is to be found in his 4th vol. p. 262. We there find, that having dissolved 17 dwts 13 grs. = 321 grs. of mercury in nitrous acid, 1½ dwt. that is, 36 grs. remained unreduced. Now, according to my calculation, 56 grs. should remain unreduced; for 100 grs. of mercury afford 12 cubic inches of nitrous air; therefore 321 grs. should afford 38,52, which contain 2,58 grs. of phlogiston, and if (according to the table) 4,56 grs. of phlogiston be necessary to metallize 100 grs. of mercury, 2,58 will be necessary to metallize 56 grs.

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of mercury: and I am satisfied, from my own trials, that more than 50 grs. would be found unreduced if dephlogisticated nitrous acid had been used in dissolving the mercury, and the solution performed with heat and a strong acid; but that which Dr. PRIESTLEY used, being the red or yellow sort, already contained much phlogiston, which contributed to the revivification of a larger quantity of mercury than would otherwise be found. It is true, that Dr. PRIESTLEY afterwards revived a great part of what originally remained unreduced; but this happened after it was for some time exposed to the free air, from which the calces of the perfect metals always attract phlogiston, as is evident in luna cornua, which blackens on exposure to the air, and hence also proceed the reductions operated by Mr. BAYEN.

But Dr. PRIESTLEY, to whose luminous experiments chemistry is already so much indebted, has been so obliging as to furnish me with some which tend more directly to elucidate the present question.

In one experiment he found that nearly 5 dwts. of minium, from whence all its air was extracted, that is, about 118 grs. absorbed 40 oz. measures of inflammable, that is, 75,8 cubic inches = 2,65 grs. of phlogiston, and were then reduced: then 100 grs. of minium should require for their reduction nearly 2,25 grs. of phlogiston. In another experiment, made with more care, he found, that 480 grs. of minium absorbed 108 oz. measures of inflammable air; according to this then 100 grs. of minium require for their reduction 1,49 grs. of phlogiston; and in two other experiments he found this quantity still less. Upon which I remark, first, that the whole of the minium was not dephlogisticated; for, besides that it is never throughout equally calcined, much of it must have been reduced

reduced during the expulsion of its air; secondly, that the quantity of the phlogiston in the inflammable air may have been greater as this varies with its temperature, and the weight of the atmosphere; so that upon the whole these experiments confirm the results expressed in the table.

*Of the affinity of metallic calces to phlogiston.*

That inflammable air or phlogiston is condensed to a very considerable degree by uniting to any metallic substance, so that its specific gravity is not only equal, but much superior, to that of the metallic earth with which it combines, may easily be concluded from the example of fixed air, which, by uniting to calcareous earth, acquires a specific gravity equal to that of gold; and hence, that metallic earth which condenses phlogiston most, and in greatest quantity, uniting to it most closely, may be said to have the greatest affinity to it; so that if we could find the specific gravity of a calx perfectly pure, both from phlogiston and fixed air, we could, by comparing its density with that of the same calx when metallized, know the density which phlogiston acquires by its union with such calx; but to procure such calces hath hitherto proved impossible, as, during their dephlogistication, they combine with fixed air, or some particles of their menstruum; and hence their absolute weight is increased, though their specific gravity be somewhat diminished. From this last circumstance it appears, that the specific gravity of calces differs much less from that of their respective metals than does the specific gravity which the phlogiston acquires by its union with those calces, from that which it possesses in its uncombined state; in the same manner as the density of quick-lime differs much less from that of lime-stone, than does the density which fixed air acquires by its



union with quick-lime from that which belongs to it in its aerial state; and hence, instead of deducing the quantity of affinity of metallic calces to phlogiston from the following proposition, *viz.* that *the affinity of metallic calces to phlogiston is in a compound ratio of its quantity and density in each metal*, I am obliged to deduce it from this other, *viz.* that *the affinity of metallic calces to phlogiston is directly as the specific gravity of the respective metals, and inversely as the quantity of calx contained in a given weight of those metals*. This latter proposition is an approximation to the former, founded on this truth, that *the larger the quantity of phlogiston in any metal is, the smaller is the quantity of calx in a given weight of that metal; and that the density which the phlogiston acquires, is as the specific gravity of the metal*. This latter proposition, however, is not exactly true, for this density is much greater; yet it is the nearest approximation I can make, and its defect is sensible only with regard to those metals which contain a considerable quantity of phlogiston, *viz.* gold, copper, cobalt, and iron: with regard to the rest it is of no importance.

Then the specific gravity of metals being as represented in the first column of the following table, the affinity of their calces to phlogiston will be as is shewn in the second column. The third column expresses these affinities in numbers homogenous with those which express the affinities of acids with their basis.

Gold

Specific gravity.				Affinity of the calces to phlogiston.	
Gold	-	19	-	0,25	1041
Mercury	-	14	-	0,147	612
Silver	-	11,091	-	0,118	491
Lead	-	11,33	-	0,116	483
Copper	-	8,8	-	0,109	454
Wismuth	-	9,6	-	0,099	412
Cobalt	-	7,7	-	0,092	383
Iron	-	7,7	-	0,090	375
Regulus of arsenic		8,31	-	0,089	370
Zinc	-	7,24	-	0,0817	340
Nickel	-	7,33	-	0,0812	338
Tin	-	7,	-	0,075	312
Regulus of antimony		6,86	-	0,074	308

Here we see, that the calx of lead has a greater affinity to phlogiston than the calces of any of the imperfect metals, and hence its use in cuppellation; for after it has lost its own phlogiston, it extracts that of the base metals, and thus promotes their calcination and vitrification.

Though the numbers in the second column express tolerably well the greater or lesser affinity of metallic calces to phlogiston, yet they have this inconvenience, that they are not homogenous with those that express the affinities of acids to other bases, which limits their use to a narrow compass, they being, on that account, incomparable with those that express the affinities of acids: I therefore endeavoured to find a coincidence between them in some one instance, in order to reduce them to the same standard, as will be seen in the next paragraph.

*Of the affinity of the vitriolic acid to phlogiston in sulphur.*

According to the principle above laid down, this affinity is in a compound ratio of the quantity of phlogiston taken up by 100 grs. of the vitriolic acid and of the density it acquires by its union with the acid. Now 100 grs. of sulphur contain 59 of acid and 41 of phlogiston, and the specific gravity of sulphur is 2,344; therefore, the loss of weight of sulphur in water =  $\frac{100}{2,344} = 42,66$  grs. the loss of weight of the acid part of the sulphur is  $\frac{59}{4,226} = 13,96$  grs.; therefore, the remainder of the loss of sulphur is the loss of the phlogistic part = 28,70 grs.; then the absolute weight of the phlogiston being 41 grs. its density will be  $1,429 = \frac{41}{28,7}$ ; and since 100 grs. of vitriolic acid take up 70 of phlogiston its affinity will be  $1,429 \times 70 = 100$ . But if the affinity of the vitriolic acid to phlogiston in sulphur had been sought in the same manner with the affinity of metallic calces to phlogiston, the quantity would be the same, though the expression of that quantity would be different, as relating to a different standard; for by that method the affinity would be directly as the density of the phlogiston, and inversely as the quantity of vitriolic acid contained in 100 grs. of sulphur, that is,  $\frac{1,429}{59} = ,024$ ; therefore, this expression answers to, and is equivalent to the former, viz. 100. By this means I formed the quantities expressed in the third column, which are homogenous to those which express the affinities of acids to their basis. Thus, the affinity of the calx of gold to phlogiston is 1041, for  $::,024 \cdot 100 :: ,25 \cdot 1041$ , &c.

The

The third point necessary for the explanation of the phenomena attending the solution of metals, and their precipitation by each other, is to determine the proportion of phlogiston which they lose by solution in each of the acids, and the affinity which their calces bear to the part so lost. I have not been able to determine this by any direct experiment; for though I might determine the part which escapes in the form of air, yet I could not that which is equally separated from the metal, but retained in the solution; yet from various collateral considerations I am induced to think, the proportion of phlogiston, separated from the metals by the different acids, is, at a medium, as expressed in the following table.

	Iron.	Copper.	Tin.	Lead.	Silver.	Mercury.	Zinc.	Witnuth.	Cobalt.	Nickel.	Reg. of antimo.	Reg. of arsenic.
By vitriolic acid	$\frac{2}{3}$	$\frac{8}{10}$	$\frac{7}{10}$	$\frac{98}{100}$	Intire	$\frac{87}{100}$	$\frac{85}{100}$	$\frac{85}{100}$	$\frac{93}{100}$	Intire	$\frac{97}{100}$	$\frac{86}{100}$
By nitrous acid	$\frac{2}{3}$	$\frac{81}{100}$	$\frac{7}{10}$	$\frac{88}{100}$	Intire	$\frac{9}{10}$	$\frac{99}{100}$	$\frac{97}{100}$	Intire	Intire	Intire	$\frac{99}{100}$
By marine acid	$\frac{42}{100}$	$\frac{57}{100}$	$\frac{3}{10}$	$\frac{6}{10}$			$\frac{6}{10}$	$\frac{7}{10}$	$\frac{94}{100}$	$\frac{8}{10}$	$\frac{84}{100}$	$\frac{8}{10}$

On this supposition the affinity of the calces to the deficient part of their phlogiston may easily be calculated; for they may be considered as acids whose affinity to the deficient part of their basis is as the ratio which that part bears to the whole, as already said. Thus the affinity of iron, thoroughly deprived of its phlogiston, being 375, as it loses  $\frac{2}{3}$ ds of its phlogiston by solution in the vitriolic acid, the affinity of iron to these two-thirds is  $\frac{2}{3}$ ds of its whole affinity, that is,  $\frac{2}{3}$ ds of 375 = 250.

These affinities, together with those of the three acids to the several calces, are represented together in the following table.

Vitriolic

	Iron.	Copper.	Tin.	Lead.	Silver.	Mercury.	Zinc.	Wismuth.	Cobalt.	Nickel.	Reg. of antimo.	Reg. of arsenic.
Vitriolic acid	270	260	138	412	390	432	318	250.310	360	320	200	260
Calx to phlog.	250	360	218	483	491	532	298	350	300	338	300	320
Nitrous acid	255	255	120	363	375	416	304	290	350	300	194	255
Calx to phlog.	250	363	218	424	491	552	337	400	383	338	308	366
Marine acid	265	265	130	400	420	438	312	250.320	370	275.310	198	290
Calx to phlog.	165	260	104	290	491	500	200	280	360	265	240	300

The affinities of calces to phlogiston are taken at a medium ; for almost all metallic substances are capable of greater or lesser dephlogistication, according to the species, concentration, and dephlogistication of their menstruum. The more they are dephlogisticated, the greater their affinity to phlogiston ; and, in general, the less their affinity to the mineral acids. Yet there is a point of dephlogistication at which the attraction of acids to the calces is strongest : thus the vitriolic acid attracts bismuth most strongly after it has been dephlogisticated by the nitrous acid ; and the marine acid attracts both bismuth and nickel more powerfully, when dephlogisticated by the nitrous or vitriolic acids.

From these *data* we may easily conceive, in most cases, what will happen on putting one metal into the solution of another.

Thus, if a piece of copper be put into a saturate solution of silver, the silver will be precipitated ; for the balance is in favour of the divellent powers.

Quiescent

Quiescent affinities.		Divellent affinities.	
Nitrous acid to silver	375	Nitrous acid to copper	255
Calx of copper to phlogiston	363	Calx of silver to phlogiston	491
Sum of the quiescent affinities	738	Sum of the divellent	746

The solutions must be nearly saturate, else a large quantity of the added metal will be dissolved by the free acid, before any precipitation can appear; yet it must not be intirely saturate, at least in some cases, as will presently be seen.

I said in *most cases*, because in some, particularly where mercury, bismuth, cobalt, regulus of antimony or arsenic, are used, another power intervenes which has not yet been fully investigated, *viz.* the attraction of calces to each other, which I shall occasionally mention.

It is worthy of observation, that the precipitating metals are more dephlogistified by this means than by direct solution in their respective menstruums, and are even dissolved by menstruums that would not otherwise affect them; because their phlogiston is torn from them by two powers instead of one: thus, though copper be directly soluble in the vitriolic acid, only when this acid is concentrated and heated to a great degree, yet if a piece of copper be put into a dilute cold solution of silver or mercury in the vitriolic of acid, or even into a dilute solution of iron, exposed to the open air, it will be dissolved; a circumstance which justly excited the wonder of Mr. MARGRAAF and Mr. WENZEL, who did not apprehend the theory of it: and hence we see how vitriol of copper may be formed by nature, and why it always contains a mixture of iron.

*Of solutions in the vitriolic acid.*

This acid dissolves iron and zinc, without the assistance of heat; because its affinity to their calces is greater than the affinity

nity which these calces bear to that portion of phlogiston which they must lose before they can unite to the acid, as may be seen by inspecting the table; but all other metallic substances unite to this acid only where it is concentrated and heated.

*Of solutions in the nitrous acid.*

The nitrous acid has less affinity to all metallic substances than either the vitriolic or marine. It has also less affinity to them than they have to that portion of phlogiston which they must lose before they can unite to it; yet it dissolves them all (gold and platina excepted) even without the aid of heat because it unites itself to phlogiston unless too dilute; and the heat produced by its union with phlogiston is sufficient to promote the solution.

But if it be too concentrated, it will not act either on lead or silver, without the assistance of heat, as BOYLE and BOERHAAVE have remarked \*; for the difference betwixt its affinity to these metals, and that of these metals to the portion of phlogiston which they must lose before they can unite to it, is very great; and when it is very concentrated, the liquor does not contain fire enough to throw the phlogiston and it into an aerial form, and reduce the solid to a liquid; the same would probably be observed with regard to mercury, if it had not been already in a liquid state. STAHL has also remarked, that it produces very little heat in dissolving silver, and none in dissolving lead or mercury †. This is easily explained, now that we know that silver contains but little phlogiston, and lead much less, the heat being evidently produced, according to the

\* I SHAW'S BOERH. 508. 2 SHAW'S BOYLE 335.

† STAHL sur les sels 168.

law discovered by Dr. CRAWFORD, by the union of phlogiston to the acid, for metallic calces produce no heat. As to mercury, the observation is not exact, for its solution is accompanied with heat, as Mr. LAVOISIER has observed, 1 LAVOIS. 248.

*Of solutions in the marine acid.*

This acid is known to dephlogisticate metals less than any other. Where the portion of phlogiston, necessary to be separated, is more strongly attracted than the acid itself, it can operate no solution, or at least very slowly, without the aid of heat; nor even where the attraction of acid is stronger to the calx than that of the portion of phlogiston it separates, if the proportion of acid to such calx be very small; because so small a quantity of acid does not contain fire enough to volatilize the phlogiston; and hence heat is necessary for the solution of lead in this acid. The dephlogisticated acid acts more powerfully.

*Of precipitations of and by iron.*

The mutual precipitations of iron and copper from the vitriolic acid by each other, have been well explained in a general manner by Mr. MONNET and Mr. BERGMAN; I shall here shew the reason of these precipitations more distinctly.

If a piece of copper be put into a saturate solution of iron, fresh made, no precipitation will happen, nor will any of the copper be dissolved in twelve hours, nor even in a longer time, if the access of air to the solution be prevented; but if the solution be exposed to the open air, the addition of a volatile alkali will shew the copper to have been acted upon in 24 hours, or sooner if heat be applied, and a calx of iron is precipitated. The operation of the affinities in the first case is as follows.



Quiescent.		Divellent.	
Vitriolic acid to calx of iron	270	Vitriolic acid to copper	260
Copper to its phlogiston	360	Calx of iron to phlogiston	250
Sum	630	Sum	510

Hence, in this case, no decomposition can happen; but in the second case, much of the phlogiston of the solution of iron having escaped, the affinity of the calx of iron to acid is diminished, and that to phlogiston is increased, and therefore the quiescent affinities may be supposed,

		and the divellent.	
Vitriolic acid to the calx of iron	240	Vitriolic acid to copper	260
Copper to its phlogiston	360	Calx of iron to phlogiston	370
	600		630

But from the increased affinity of the calx of iron to phlogiston it might be inferred, that as the iron recovers its phlogiston, the acid should re-act upon it and quit the copper; and this would certainly happen, if it recovered its phlogiston in sufficient quantity, but the access of air and heat prevents its retaining it, at least in sufficient quantity.

This increased affinity of the calx of iron to phlogiston is not a mere supposition; for, if into a solution of iron, so far dephlogisticated as to refuse to crystallize, some fresh iron be put, the impoverished calx will re-attract so much of the phlogiston given out during the solution of the fresh iron, that it will now afford crystals, as Mr. MONNET has observed in his excellent Treatise on Vitriolization. The diminished attraction of the calx of iron to acids is also evident from this experiment,

and also from the necessity of adding more acid to a turbid solution of iron, in order to re-establish its transparency. The calces of copper also precipitate a dephlogisticated solution of iron, as they should, the affinity of the acid to such calx of iron being 240, and that to copper being 260. With regard to the solution of iron in nitrous acid, the same thing happens; but as this solution contains a large excess of acid, a portion of copper is dissolved even before any of the iron is precipitated.

With regard to a solution of iron in the marine acid, though exposed to the open air, copper precipitates nothing from it in 24 hours.

But if a clean piece of iron be put into a solution of copper in the vitriolic acid, the copper is immediately precipitated; for here the quiescent and divellent affinities exhibited in the first scheme are reversed, the quiescent becoming the divellent, and *vice versa*. It is needless to add, that copper is in the same manner precipitated by iron from the nitrous and marine acids.

Hence the practice of extracting copper from some mineral waters by means of iron. These waters, therefore, furnish afterwards, by evaporation, vitriol of iron; but it is remarkable, that this vitriol is much paler than the common, and less fit for dying, 2 SCHLUTTER 507. The reason of which is, that it is more dephlogisticated, not only because old iron is chiefly used, but because copper, containing more phlogiston than an equal weight of iron, deprives it of more of its phlogiston than it would lose if barely dissolved in the vitriolic acid.

Cast iron, according to SCHLUTTER, will scarcely precipitate a solution of copper; and in effect Mr. BERGMAN has found that it contains less phlogiston than bar iron.

I have always found silver to be easily precipitated from its solution in the nitrous acid by iron. The sum of the quiescent affinities being 625, and that of the divellent 746; yet Mr. BERGMAN observed, that a very saturated solution of silver was very difficultly precipitated, and only by some sorts of iron, even though the solution was diluted, and an excess of acid added to it \*; the reason of this curious phenomenon appears to me deducible from a circumstance first observed by Mr. SCHEELÉ, in dissolving mercury, namely, that the nitrous acid, when saturated with it, will take up more of it in its metallic form †. The same thing happens in dissolving silver in the nitrous acid in a strong heat; for, as I before remarked, the last portions of silver thrown in afford no air, and consequently are not dephlogisticated. Now this compound of calx of silver, and silver in its metallic form, may well be unprecipitable by iron, the silver, in its metallic form, preventing the calx from coming in contact with the iron, and extracting phlogiston from it: and hence also, iron has sometimes been observed not to precipitate a solution of mercury in this acid ‡.

It has been long thought, that iron may be precipitated from acids by zinc, though NEWMAN long ago denied it; but Mr. BERGMAN has satisfactorily cleared up this point, by shewing that zinc cannot precipitate iron from the vitriolic acid, until the solution of iron loses part of its phlogiston. With regard to the nitrous acid, I found, that zinc does not precipitate iron; but, on the contrary, iron precipitates zinc; but in a short time the acid re-dissolves the zinc, and lets fall the iron, which evidently proceeds from the too great dephlogistication of the calx of iron. But zinc precipitates iron from the marine,

\* Dissert. de Phlog. Quantitate in Metal. p. 6.

† 39 SVENSK. Handling, p. 70.

‡ 2 CRELL. Nev. Entdeck. p. 266.

though

though with difficulty; for after 24 hours the galls still struck a black. I should also add, that iron does not precipitate zinc from the vitriolic acid.

Most metallic substances, precipitated by iron from the nitrous acid, are in some measure re-dissolved shortly after, as the nitrous acid soon dephlogisticates the iron too much, then lets it fall, and re-acts on the other metals and re-dissolves them.

The precipitation of the argillaceous earth from allum by iron is owing to the excess of acid in the allum which first dephlogisticates the iron; and when this is dephlogisticated, it attracts the acid more strongly. Earth of allum, on the other hand, precipitates iron when the solution of iron is dephlogisticated by heat. It may also produce this effect by depriving iron of its excess of acid which keeps it in solution.

*Of precipitations of and by copper.*

When silver is dissolved in the nitrous acid, and a piece of copper is put into the solution, it sometimes happens, that the silver is not precipitated, as Dr. LEWIS has observed\*. This happens either when the nitrous acid is supersaturated with silver having taken up some in its metallic form, as already observed; or when the silver is not much dephlogisticated, for then its affinity to phlogiston, which is the principal cause of its precipitation, is less than 491; therefore, the remedy is to heat it and add more acid, by which it is dephlogisticated further. However, the nitrous acid always retains a little silver. SCHLUTTER. 362. Hist. Mem. Par. 1728.

It is commonly said, that if filings of copper be put into a boiling solution of allum, vitriol of copper will be found, and

\* *Commercium Philos.* p. 157.

the

73     *Mr. KIRWAN's Experiments and Observations on*  
the earth of allum precipitated. If this were true it would be very surprising, as copper is soluble, even with the assistance of heat, only in the concentrated vitriolic acid. Hence I made the experiment, and found, that after 20 hours boiling not the smallest particle of copper was dissolved, for the colour of the solution was not altered by volatile alkalies: and though the allum was precipitated, it still retained its saline form, so that it lost only its excess of acid in this experiment.

*Of precipitations of and by tin.*

Tin is not precipitated, in its metallic form, by any metallic substance; and the reason is, because its precipitation is not the effect of a double affinity, but of the single greater affinity of its menstruum to every other metallic earth. Metals that are precipitated from the nitrous acid by tin, are afterwards redissolved, because the acid soon quits the tin, it becoming too much dephlogisticated.

*Of precipitations of and by lead.*

Metals dissolved in the vitriolic and marine acids, and precipitable by lead, according to the indication of the balance of affinities, are yet slowly precipitated, because the first portions of lead that are dissolved form salts of difficult solution, which cover its surface, and protect it from the further action of the acid; and yet it contains so little phlogiston, that a great deal of it must be dissolved before it gives out enough to precipitate the dissolved metals.

Mr. BERGMAN observed, that a very saturated solution of lead is difficultly, if at all, precipitable by iron. Does not this also arise from some lead being taken up in its metallic form? Iron does not precipitate lead from the marine acid, though a  
pre-

precipitate appears ; for this precipitate still retains the marine acid : on the contrary, lead precipitates iron from this acid, though very slowly.

*Of precipitations of and by mercury.*

Though the difference betwixt the quiescent and divellent powers be very small, yet mercury is quickly precipitated from the vitriolic acid by copper ; because the attraction of the calx of mercury to phlogiston is very strong, and a very small proportion of that contained in copper is sufficient to revive it.

Silver does not precipitate mercury from the vitriolic acid, unless it contains copper, and then it does precipitate it ; yet if silver and turpeth mineral be distilled, the mercury will pass in its metallic form, WENZEL 42. ; which shews that the affinity of calx of mercury to phlogiston is increased by heat. The difference betwixt the quiescent and divellent powers is indeed very small.

Silver appeared to me to precipitate mercury from the nitrous acid, though very slowly, when the solution of mercury was made with heat, and not over saturated ; but when the solution of mercury was made without heat, it was not at all precipitated. On the other hand, mercury precipitates silver from this acid, not by virtue of the superiority of the usual divellent powers, but by reason of the attraction of mercury and silver to each other, for they form partly an amalgama and partly vegetate, and scarce any of either remains in the solution. The same thing happens, that is, they vegetate, if solutions of both metals in the same acid be mixed together.

Silver does not precipitate mercury from the solution of sublimate corrosive ; but, on the contrary, mercury precipitates silver from the marine acid : and if a solution of horn silver in volatile alkali be triturated with mercury, the silver will

will be freed from its acid and calomel formed, 1 MARGRAAF 284.; and yet, if calomel and silver be distilled, the mercury will pass in its metallic form, and horn silver will be formed, *ibid.* 26. The same thing happens if silver and sublimate corrosive be distilled, 1 POTT. 338. STAHL *des sels*, 306; the affinity of calx of mercury to phlogiston increasing with the heat.

*Of precipitations of and by bismuth.*

With respect to the vitriolic acid I have made the sum of the quiescent and divellent powers equal, though in fact sometimes the one preponderates and sometimes the other. Wismuth precipitates nothing from vitriol of copper in 16 hours; nor does copper from vitriol of wismuth. Copper is said to precipitate wismuth from the nitrous acid; but I have also seen copper precipitated from this in its metallic form by wismuth. The variations proceed from the different dephlogistication of copper.

*Of precipitations of and by nickel.*

Unless nickel be pulverised it scarcely precipitates any metal.

Zinc precipitates a black powder from the solution of nickel in the vitriolic and nitrous acid, which Mr. BERGMAN, by a method peculiar to him, has shewn to consist of arsenic, nickel, and a little of the zinc itself. The arsenic attracting the calx of nickel\*; but zinc precipitates nickel from the marine acid.

The solution of iron in vitriolic acid acts on nickel, and that of nickel in this same acid acts on iron; but neither precipitates the other in 24 hours; but on longer rest, iron seems to have the advantage; but iron clearly precipitates nickel

\* 1 SUENSK. Handling. 1780.

from the nitrous acid; and though nickel seems also to precipitate iron, yet this arises only from the gradual dephlogistication of the iron.

Nickel precipitates copper in its metallic form from the vitriolic acid. It also precipitates copper from the nitrous and marine acids; but copper precipitates arsenic from a nitrous solution of nickel. The vitriolic and nitrous solutions of lead seem to act in specie on nickel, that is, to dissolve it without any decomposition, the calces uniting to each other. The vitriolic and nitrous solutions of nickel for some time act on lead in the same manner; but at last nickel seems to have the advantage. With regard to the marine acid, lead seems to have the advantage, though a black precipitate is seen, whichever of them is put into the solution of the other.

Nickel readily precipitates wismuth from the vitriolic and nitrous acids; but as to the marine I found each of these semi-metals soluble in the solution of the other, yet nickel precipitates wismuth very slowly, and only as to part; and wismuth precipitates a red powder, which I take to be ochre, from the solution of nickel.

Nickel and tin are slightly acted on, each by the salt of the other; but the precipitations are as indicated by the balance of affinities.

*Of precipitations of and by cobalt.*

Cobalt is not precipitated either from the vitriolic or nitrous acid by zinc; but it seemed to me to be precipitated by zinc from the marine acid.

Though iron precipitates cobalt from the three acids, yet I found much of the cobalt retained both by the vitriolic and nitrous acids, particularly the latter, which, after letting fall



the cobalt, afterwards re-takes it, and lets fall the dephlogisticated calx of iron.

Nickel also, though it does not precipitate cobalt itself, as appears by the remaining redness of the solution, yet constantly precipitates some other heterogenous substance from it. The solution of cobalt in the marine acid becomes colourless by the addition of nickel.

Wismuth is soluble in the vitriolic and nitrous solutions of cobalt, and causes a small white precipitate, but does not affect the true cobaltic part. These solutions in vitriolic acid cannot be attributed to an excess of acid, as they are made in a dilute acid, and without heat. Copper also precipitates a white substance from the nitrous solution of cobalt, which I take to be arsenic.

It is difficult to procure either nickel or cobalt very pure; it is evident those I used were not so.

### *Of precipitations of and by regulus of antimony.*

Copper neither precipitates, nor is precipitated from, the vitriolic acid by regulus of antimony, at least in three days; but vitriol of antimony in specie dissolves it slowly.

The regulus is also acted upon by vitriol of lead, for it becomes red after remaining 16 hours in the solution of that vitriol; and lead scarcely precipitates it from the vitriolic acid.

I also found, that powdered regulus precipitates vitriol of mercury very slightly.

Wismuth neither precipitates, nor is precipitated by, this regulus from the vitriolic acid in 24 hours.

Though tin precipitates this regulus from the nitrous acid, yet if the regulus be put into a solution of tin in this acid, in 16 hours neither will be found in the solution, either  
by

by reason of the dephlogistication, or of the union of the calces to each other.

Iron does not precipitate this regulus intirely from the marine acid, but a triple salt seems to be formed, consisting of the acid and both calces. The regulus is also soluble in marine salt of iron.

Neither does copper precipitate the regulus from marine acid in 16 hours; and if the regulus be put into marine salt of copper it will be dissolved, and volatile alkalies will not give a blue but a yellowish white precipitate, so that here also a triple salt is formed.

*Of Precipitations of and by regulus of arsenic.*

The solutions of arsenic act in most cases like two acids: thus iron, copper, lead, nickel, and zinc, are acted on by vitriol of arsenic (that is, its solution in vitriolic acid) but scarce give any precipitate.

Neither does iron precipitate arsenic from the nitrous acid, but copper does, and even silver gives a slight white precipitate; but regulus of arsenic precipitates silver completely in 16 hours. Hence the former precipitate seems to be a triple salt.

Mercury also slightly precipitates arsenic from the nitrous acid, and seems to unite to it, yet is itself precipitated by regulus of arsenic in 24 hours.

Wismuth forms a slight precipitate in the nitrous solution of arsenic; but regulus of arsenic forms a copious precipitate in the nitrous solution of wismuth; so that I believe the calces unite.

Nickel does not precipitate arsenic from the nitrous acid, but both calces unite; but regulus of arsenic produces a copious precipitate in the nitrous solution of nickel, yet the liquor continues

M 2

green;

green ; so that certainly the nickel is not precipitated ; the white precipitate in this case seems to be slightly dephlogisticated arsenic.

This regulus also causes a white precipitate in the nitrous solution of cobalt, but the liquor still continues red.

With regard to the marine acid, copper precipitates the regulus, but volatile alkalies do not strike a blue with this solution, which shews the copper unites with the arsenic. Iron also precipitates the arsenic. Tin is soluble in marine solution of arsenic, but I could observe no precipitate, nor does regulus of arsenic precipitate tin.

Neither wismuth nor the regulus of arsenic precipitate each other from the marine acid in 16 hours. Regulus of antimony is also acted upon by the marine solution of arsenic, though it causes no precipitate, nor does the regulus of arsenic precipitate it.

IV. *A Description of a Species of Sarcocoele of a most astonishing Size in a Black Man in the Island of Senegal; with some Account of its being an endemial Disease in the Country of Galam. By J. P. Schotte, M. D.; communicated by Sir Joseph Banks, Bart. P. R. S.*

Read December 19, 1782.

**T**HERE are certain diseases which are peculiar to certain countries only, and are thence called endemial ones of such particular countries where they occur. The more progress we make in the discoveries of countries, the more we are convinced of this fact, and the greater is the number of those diseases that become known. Their formation may depend on climate, food, water, hereditary disposition, and other causes. Many endemial diseases of the most distant countries have been described by ingenious travellers; but as the Europeans have not yet penetrated into the interior parts of many countries, it is probable, that there may be several more of this kind, entirely unknown to us. A disease of this class, which I have seen at Senegal, and which, as far as I know, has not yet been mentioned by any author, convinces me of what I have advanced; and as it is a remarkable one, I think a short description of it may not be unacceptable to the curious in physic.

Mr. BISHOPP, surgeon in chief of the province of Senegambia (who now resides in London) telling me one day, that he was going to see a poor black man of the Bambara nation, afflicted with a  
 1 most

most extraordinary and dreadful disease in his testicles, I accompanied him, being glad of the opportunity of seeing it. We entered the hut, and saw the man lying on a negro bed, elevated about a foot from the ground. He said to Mr. BISHOPP, that there was again an ulcer on his scrotum, which had made him take the liberty to request his attendance,. I looked at the scrotum, and found it of an astonishing size; but the place where he lay being dark, the hut having no windows, and those people having no candles, he was asked, if he could not walk towards the door, that we might see better. He answered, that he would try; but this was attended with much difficulty. A long cotton sheet was first spread on the ground before the bed, which being done, he took, with both his hands, the enormous scrotum, moved it gradually on the border of the bed, let it slide down gently, and put it into the middle of the sheet: after this he took the two ends of the sheet, passed them up the fore-part of his body, over his shoulders, and had them tied behind his neck. This being done he got up, placing the right-hand upon his right-thigh, and holding the sheet with the left-hand, and proceeded in this manner, with his knees a little bent, slowly towards the door, partly sliding the scrotum on the ground, and partly supporting it with his neck by means of the sheet. I was astonished at its enormous size, when I saw it in the light, and yet I neglected to measure it, thinking at the time, as is often the case, that I should have opportunities enough to do it; but the sudden invasion of the island by the French prevented me afterwards from performing it. However, according to my guess, and without any exaggeration, the whole mass might be about two feet and a half long from the os pubis to its lower extremity, and about eighteen inches in diameter across from thigh to thigh.

Its

Its weight I will only state at fifty pounds, as it was estimated by Mr. BISHOPP, though I believe it to have been more, and indeed from its dimensions, and from its being a solid mass, it must certainly have exceeded that weight. It was of an oblong form, and resembled in some measure the shape of the scrotum of a bull. It felt very hard to the touch, and the skin of it was so tight, that it could not be pinched by the fingers. The penis was quite hid in the bulk, as generally happens when the scrotum is much extended, and may be easily comprehended by those who have seen large ruptures. The skin of the perinæum and of the abdomen was drawn downwards, the navel being nearer to the os pubis than it is in the natural state. There was a large aperture formed by the skin about a foot downwards from the os pubis, rather inclining towards the right-side, out of which the urine came, which, however, did not run in a stream, but came irregularly from all the interior sides of the aperture. When he made water, he inclined the mass, which rested on the ground, a little forwards, and he held a wooden bowl close underneath the aperture, into which the urine was immediately received, that it might not run along the mass, and occasion excoriation.

There was an ulcer on the anterior part of the scrotum, rather towards the left-side, of about two inches long, and one inch broad and deep. He said, that it had begun with a pustule or boil, which being broke had gradually increased to this extent. The pus which came from it was white, thick, and of a good kind. The bottom of it was red, and, when touched with the probe, gave him very acute pain. The edges of it were not very callous, and in appearance it did not much differ from an ulcer of a good kind in any other fleshy part of the body. No other remedies were applied to it but those generally

used

used in common ulcers. It was filled up from the bottom with lint; a pledget of basilicum was put over it, and the edges were now and then touched with blue vitriol. By those means granulations began to shoot from all sides, the sore filled up gradually, and a cicatrix was formed. He had had smaller ulcers of this kind in other parts of the scrotum before this time, which, Mr. BISHOPP told me, he had treated with the same success.

The man was rather thin than fat, and might be about fifty years old. He himself, like most blacks, did not know his age; and if he had pretended to know it, I might, perhaps, not have believed him: for as old age is much respected among those people, they are very apt, when they are once passed fifty, and have grey hair, to call themselves older than they really are, in order to command respect. His abdomen seemed rather empty, and appeared drawn in towards the spine; yet I do not think, that any of the intestines had descended into the scrotum, or if any had passed down, the annuli of the abdomen must have been so dilated as not to occasion the least obstruction in them; for he never had, to my knowledge, any of those complaints or symptoms which attend ruptures. Besides this, it is to be observed, that ruptures are not very common among the blacks about Senegal; indeed I can say, that I never saw one of them.

Having thus far given an account of what I saw myself of this remarkable disease, I shall now relate what I have been credibly informed of by other people concerning its beginning and progress. The man had been purchased up the river as a slave, when he was about the age of puberty, and brought down to Senegal, where he was kept as a house-servant by an opulent inhabitant. He was for some years healthy and well; but afterwards

afterwards his testicles began to swell insensibly; without inflammation, pain, or any other inconvenience. They increased gradually, though slowly, and became some years after of such a bulk, that he was neither able to walk nor perform his usual work. That he might, however, not be quite idle, as he was otherwise a stout and able fellow, he used to cut bars of iron into pieces of a foot long, which bear a certain price at Senegal, and go among the blacks like current money. This he could do sitting with a chisel and hammer, and a small anvil placed before him on the ground, his legs bent under him, and the big scrotum resting on the ground. Mr. BISHOPP had seen him perform this work for many years; at last, however, the scrotum increased to such a degree, that the great bulk prevented him from doing it any longer. From the time that the disorder had first begun to shew itself to the time I saw him, five and twenty years had elapsed; he was alive when I left the island in February, 1779, and may be so now.

This man was the one I ever saw afflicted with this disease at Senegal; but I am credibly informed, that it is endemial in a country which goes, among the blacks at Senegal, by the general name of Galam, and of which this man was a native. This country lies east of Senegal, at the distance of about nine hundred English miles, and its inhabitants are called Bambaras: I have been told by those inhabitants of Senegal, who go annually in the rainy season in a fleet of small craft to Galam for trade, that this disease is particularly common among the chiefs or noblemen of that country, who are stiled in their own country language Batcherees; and that they have large wooden bowls, fixed on the fore-part of the saddle, into which they place the big scrotum when they take a ride on horse-back. Though this latter circumstance seem a little romantic, yet as



it has been related to me, not by one but by many, separately and at different times, I give it credit, and I have not the least hesitation to believe, that the disease is common there. Many of the inhabitants of Senegal have applied to me previously to their setting off for that country, and asked me, if I could not give them medicines which would cure that disorder, with a promise, that if they proved successful, I might be sure of a very ample reward of gold; but the improbability of succeeding in the reduction of such enormous masses to their pristine state prevented me from giving them any.

When I was at Fort James in the river Gambia, for a short time in the year 1776, I was told by some Marahbuts, or Mahometan priests, of the Mandinga nation, that this disease was now and then to be met with among the chiefs of their nation, and that they knew no cure for it\*. I have no reason to discredit

\* It is to be observed, that those Marahbuts apply themselves, besides religious matters, to the study of physic; but only as far as it rests on experience alone, without entering into the investigation of the causes of diseases. They are also often called upon by the kings and chiefs to give their opinion in points of law and equity. Most of them are well-versed in the Arabic language of the Mauritanic dialect, and they are the only people of letters among the blacks; for none of the black nations about Senegal and Gambia have even an alphabet, much less any writings in their own languages. I believe the selling of charms constitutes the greatest part of their revenue: and the more reputation one of them has acquired, the dearer he sells them. Those charms usually consist in nothing but a few lines taken from the Koran, written on a little piece of paper, which, after being sewed up very nicely in leather or cloth, the buyers wear about their bodies. They are to defend and protect them in dangers; but, as one charm has only the power of protecting them against one single kind of danger, they are obliged to have a great many of them, in order to have a protection against every probable danger that may befall them; hence many of the blacks are covered with them in different parts of the body; and they have such a strong faith in them,

discredit their assertion, and what makes it more probable to me is, that the Mandinga and Bambara nations seem to be nearly related to one another in outward appearance, customs,

them, that when they are surprized in the night-time by an enemy, they will not take up arms for their own defence, though in the most imminent danger, till they have dressed themselves with those charms; and then they will meet him undauntedly. This faith in charms, however, is a corruption of the Mahometan religion, and the Moors, who live on the north-side of the river Senegal, observing it in its purity, make no use of them. The Marahbuts of the black nations, as well as those of the Moors, are also the principal merchants and the most opulent people among them, and the gum trade on the river Senegal is chiefly carried on by those of the Moors. The Marahbuts are also the only people who can travel with any safety into distant kingdoms, which no layman can well do without running the risk of being made a slave. Their religious profession protects them every where; they are even respected among those nations who are not Mahometans; and they are considered by them as godly and virtuous people, and men of wisdom. They make proselytes in the Mahometan religion every where; and I am inclined to believe, that they will extend and spread it in time all over Africa. I have seen some Marahbuts of the Pool or Fool nation at Senegal who were pretty well versed in the old testament, and knew partly the history of the institutor of the new one. One day as I was talking with them on the writings of Moses, happening not rightly to recollect the lineage from Adam to Abraham, one of them flattened the sand, made it even, and drew with his fingers on it the genealogy from Adam down to Jacob, which, to the best of my recollection, corresponded with that given by Moses. While he was doing this, I looked at him with pleasure and satisfaction, because it resembled so much the rude simplicity of the early ages. The Marahbuts reason in general exceedingly well on such subjects as they are acquainted with, but they have a way, like the eastern nations, of adducing parables or similes in their arguments which do not always bear the strictest resemblance to the case in hand, though they are very persuasive with such people as are not capable of investigating the points in which they differ from the case in question. I was always much delighted with their conversation, and was often sorry that I was not master of their different languages, and able to converse with them without an interpreter. The Marahbuts of the Moors are more learned and ingenious in every respect than those of the black nations; but I had not much opportunity of conversing with them, as they were not allowed to reside on the island.

and language, though not entirely in religious matters; for many of the Mandingas are Mahometans, which the Bambaras are not. Their languages resemble one another so nearly that a Bambara from Galam, and a Mandinga from the kingdom of Barra, which extends from the sea coast along the north-side of part of the river Gambia, can partly understand one another. Both nations have also a custom of marking their children in various manners by incisions in the skin, and that of filing their fore teeth (incisores) till they become quite pointed, which I imagine they consider as being handsome.

As the disease, according to the information I received, begins with a gradual swelling of the testicles without any pain or inflammation, I am inclined to consider it as a farcocele. HEISTER, in his Surgical Institutions, says, that the disease begins and increases mostly in the same manner, when it affects the testicles themselves; but that he never saw any of them much bigger than a man's fist. This difference in the size does, in my opinion not, alter the disease; for we know, that the Bronchocele is hardly known in some countries, that it is of a moderate size in some others, and that in others again it has been seen to increase to such an enormous bulk as to hang down over the breast and belly; yet this difference of size does not alter the nature of the disease, and it still retains the same name.

It is difficult to point out the causes of such a farcocele, as consists in the spontaneous tumefaction of the testicles themselves; neither do I find any satisfactory ones assigned by the author I have just now quoted; and as I have not been in Galam, I can hardly say any thing probable concerning those of the disease I have described, I shall, however, suggest the following.

As polygamy is lawful and customary among the Bambaras as well as among all the other nations about the river Senegal and Gambia, and as the riches and consequence of a man are estimated by the number of wives that he keeps, the chiefs of the people have always a great number of them. I have been told, that the Batcherees of Galam have their victuals most immoderately seasoned with Cayenne pepper; and I know myself, that the opulent people of the Mandinga nation make the same abuse of it. This may, perhaps, be done with a view to its operating as a provocative; for it has a peculiar effect on the seminal vessels, and will produce erections, attended with a dull pain and turgescency in the testicles: I was therefore inclined to think, that the immoderate use of this pepper might partly be the cause of this disease; but then again this could not be the case in the man I saw at Senegal, where none, or at least very little of it, is used.

The most probable cause of it seems to be an hereditary disposition; for, as it only begins to shew itself about the age of twenty-five or thirty, a man may be father of a great many children before it takes place, and as it seems to be confined to families of the principal people of the Bambara nation, it may be, that the man I saw afflicted with it at Senegal was descended from such a family, and made a slave in his younger years by some fatal accident or other, as is often the case in those countries.

The French, who are the present possessors of the river Senegal, may perhaps be able to give shortly a more perfect account of this remarkable endemial disease. In the meantime, if this should be deemed worthy the notice of the Royal Society, it will afford the greatest satisfaction to him, who has the honour to be, &c.

V. *A Description of a new Construction of Eye-glasses for such Telescopes as may be applied to Mathematical Instruments.*  
*By Mr. Ramsden ; communicated by Sir Joseph Banks, Bart.*  
*P. R. S.*

Read December 19, 1782,

**T**O correct the errors in eye-glasses, arising from their spherical figure, and also from the different refrangibility of light, it has been held absolutely necessary to have two, placed in such manner, that the image formed by the object-glass of the telescope should be between them; but in those telescopes that are applied to mathematical instruments, the interference of the first eye-glass before the image is formed is productive of many bad consequences; should that eye-glass have the least shake or motion whatever, it totally alters the adjustment of the instrument; and the diminishing also of the image by this position, obliging us to shorten the focus of the nearer eye-glass, the wires in the focus of the telescope are thereby considerably more magnified than they would have been with the same power, had both the eye-glasses been put between the image and the eye.

Many defects in the micrometer with moveable wires are caused by the construction of the eye glasses of the telescope to which it is applied. If only one eye-glass is used, the field is so contracted, that it is impossible to measure the diameter of the sun or moon with precision, if the telescope magnifies above 30 times;

times; and if, to enlarge the field, we use the present construction of two eye-glasses, the consequence is yet worse; because equal spaces between the wires will not then correspond to equal spaces on the objects it represents, as those conversant in the theory of optics well know; and this inequality depending on the form, position, and refractive power of the first eye-glass, it will be impossible to have data sufficiently exact to allow for that error.

Those who were sensible of this defect have thought to correct it by the application of an achromatic eye glass, on the principle of that kind of object-glass, not supposing it possible to correct the aberrations from the different refrangibility of light, and also from the spherical figure of the lenses by any other means than combining a concave lens with the convex ones; but the violent and contrary refractions from the necessary large size of the lenses in proportion to their focal lengths, not only occasioned great loss of light, but rendered it impossible to correct the spherical aberration so as to obtain an angle of vision much larger than could be had by a single eye-glass: yet, however absurd it may have appeared to attempt correcting both aberrations, when the lenses are both convex, and are on the near side of the wires, the following observations will shew the practicability of it, and may throw some light on the theory of eye-glasses which seems hitherto not well understood.

Sir ISAAC NEWTON has shewn in his *Lectiones Opticæ*, in that section *De Phænomenis lucis per prisma in Oculum transmissæ*, that the appearance of colours on the edges of objects when viewed through a prism depends on the proportion of the distance between the prism and the object, compared with that between the prism and the eye, that is to say, the nearer the object is brought

brought to the prism, the less will be the border of colours on the contours of the object.

To apply this to practice I placed a plano convex lens *a* (vide fig. 1.) with its plane side near an object, or an image IN formed by the object glass of a telescope, and thus magnified the image which, from the position of the lens, was sensibly free from colour; but the respective foci of a lens so placed being very near each other, and on the same side, the emergent pencils diverge on the eye, and give indistinct vision: this was remedied by placing a second lens *b* a little within the focus of the former, the combined foci of the two lenses being in the place of the image, the rays were thereby made to fall parallel on the eye, and to shew the object IN distinctly. If, by putting the lens *a* very near the image, any imperfection in it becomes too visible, that distance may be considerably increased, without producing any bad effect; for theory, as well as experiment, shews, that a small aberration from the different refrangibility of light is of little consequence compared with the same quantity of aberration caused by the spherical figure of the lenses, but even that colouring may be corrected in the nearer eye-glass: for let a ray (fig. 2.) from an object *o*, by passing through a lens B, be separated into colours, *ac* being the direction of the violet rays, and *at* that of the red; if another lens be put at *c*, the violet rays passing through its center will suffer no refraction, while those of the red, passing at some distance from thence are refracted, and the emergent red and violet will be parallel, when the mean refracting angle of the lenses at the incidence of each pencil are to one another inversely as the diameters of those pencils.

If we attend to this position of the eye-glasses, it will be found equally advantageous for obviating the spherical aberration

of

of an oblique pencil as that from colour. In both, where there is a necessity for having a large portion of a sphere, we have only to make the pencil on such lens as small as possible, and we may regulate the direction of the rays in each pencil at pleasure when they approach the axis of the telescope.

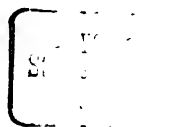
To illustrate this, let us compare the effect of the spherical aberration of a lens on an oblique pencil in this position with that produced by the same lens, placed as usual at its focal distance from the image. Let AC, fig. 3. represent the semi-object-glass of a telescope, CT its axis, E an eye-glass, and F the common focus of both the object-glass and eye-glass. Let AFC be an oblique pencil of homogeneous rays, G and H the points where the axis and the extreme rays pass through the eye-glass: the aberration of this pencil from the spherical figure of the lens E will be  $EG' - EH'$ ; but as the lens approaches towards F, EG and GH, becoming equal, this cause of aberration vanishes accordingly. The effects of the lens *k* will be altogether insensible from the smallness of its aperture; or it might be corrected in the figure of the object-glass, by making its aberration negative as much as this is affirmative.

It has been usual to consider that form and position of the eye-glasses best that would make the pencils from every part of the field intersect each other in the axis of the telescope at the place of the eye; but this will be found of little consequence, seeing the diameter of a pencil here is generally much less than the pupil, nothing more is requisite than that the eye may take in the pencils from the different parts of the field at the same time: but the field of a telescope will be most perfect when the construction of the eye-glasses is such, that the focus of an extreme and of central pencil are at the same distance

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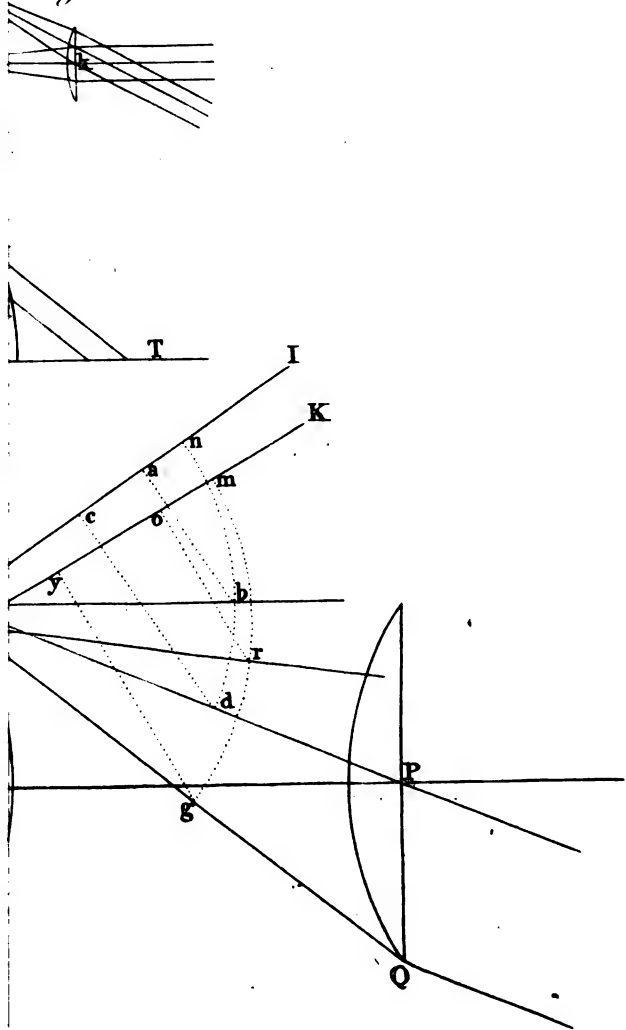


from the eye. The disposition above described will be found conformable to that idea.

Let  $AB$  (fig. 4.) represent an image formed by the object-glass of a telescope,  $V$  the first eye-glass, as already described, with its plane side towards the image; let  $AC$  be the axis of a pencil of rays incident on the first surface of the lens  $V$ , and  $Ae$  an extreme ray of the same pencil. Take  $CF$  to  $CA$  as the sine of incidence out of air into glass is to the sine of refraction, and  $F$  will be the focus of this pencil after passing through the first surface of the lens  $V$ . From the point  $F$  draw the angle  $CFe$ , the incident pencil on the second surface of this lens, continue the lines  $FC$  and  $Fe$  to  $b$  and  $r$  respectively, and draw the perpendiculars  $OI$  and  $OK$  on the point  $C$ , describe the arc  $nd$ , and making  $cd$  to  $ab$ , as the sine of refraction out of glass into air is to the sine of incidence, draw  $cd$  continued till it cuts the axis in  $P$ . In like manner, on a center  $e$  describe the arc  $mg$ , and making  $yg$  to  $or$  as the sine of the angle of refraction is to that of incidence, draw the line  $egQ$ ; continue it and the line  $Cd$  backward till they meet each other in  $b$ , and it will be the focus of the emergent pencil from the second surface of the lens  $V$ . On the axis  $CF$  set off the distance  $Cds$  equal to  $Cb$ , and draw  $es$  and  $Ce$ . Now it is evident from the figure, that the focus of the emergent pencil will be nearer to  $C$  than the object itself, in the proportion the angle  $Cse$  exceeds the angle  $CAe$ .

Thus, from the great angle of incidence of the oblique pencil on the second surface of the lens, the focus of the emergent pencil is brought nearer to  $P$  the second eye-glass, while that of the principal pencil remains the same, or very nearly so; and the image will become more distinct towards the edge of the field the nearer  $Pb$  and  $PT$  approach to equality.

*Fig. 1.*





To give a proper demonstration and theorem for the exact form of the first lens, according to its distance from the image, would require more leisure than is consistent with the situation of one not very conversant with mathematics. That distance, in proportion to the focal length of the lens, so that any unavoidable defect in it may become invisible, must be determined by experiment. If any variation be made in the form of this lens, it will be better to make the plane side rather a little convex than concave. By the latter the image would be distorted by the too great obliquity of the rays near the extremity of the lens.

Thus we have a system of eye-glasses which may be taken out of the telescope, in order to wipe them at pleasure. Or the magnifying power of the telescope may be varied without affecting the line of collimation, or in any manner altering the adjustment of the instrument to which such telescopes may be applied with many other advantages. In the present improved state of telescopes too, the disagreeable appearance of the wires from the great power of the eye-glasses is in a great degree remedied. The same principle may be usefully employed in many other cases. What is herein contained is only to be considered as an explanation of this very useful construction, and which is given in hopes that some person of more abilities in the science of optics will favour us with a general theorem, in order that its application may be more universal.

VI. *Account of several Lunar Iris. By Marmaduke Tunstall, Esq. F. R. S. in Two Letters to Sir Joseph Banks, Bart. P. R. S.*

Read May 30, 1782, and January 23, 1783.

TO SIR JOSEPH BANKS, BART. F. R. S.

DEAR SIR,

**A**S I am ever happy to seize on any opportunity to express my regard to yourself, and my attention to the Royal Society, I cannot omit this occasion of acquainting you of rather an unusual phenomenon seen here on Friday night last, the 27th of February, between seven and eight, especially as it might probably be visible only at a small distance. It was an *Iris lunaris*, or *Lunar rainbow*, in tolerable distinct colours, similar to a solar one, but more faint; the orange colour seemed to predominate. I was unfortunately not a spectator myself; but can sufficiently rely on the authority, as a clergyman in my house, and some servants, on whom I can depend, observed it for near a quarter of an hour. It happened at full moon, at which time alone they are said to have been always seen. Though ARISTOTLE is said to have observed two, and some others have been seen by SVELLIUS, &c. I can only find two described with any accuracy; viz. one by PLOT, in his History of Oxfordshire, seen by him in 1675, though without colours; the other seen by a Derbyshire gentleman at Glapwell

near Chesterfield, described by THORESBY, and inserted in N<sup>o</sup> 331. of the Philosophical Transactions: this was about Christmas, 1710, and said to have had all the colours of the *Iris solaris*. The night was windy, and though there was then a drizzling rain and dark cloud, in which the rainbow was reflected: it proved afterwards a light frost. That this very imperfect account, though it may be strictly relied on, may give any satisfaction to you, or the gentlemen of the Society, would be the occasion of great pleasure to

DEAR SIR, your much obliged, &c.

Wycliffe near Greta Bridge, Yorkshire,  
March 1, 1782.

The particular circumstance, which appeared extraordinary to THORESBY, of the bow being nearly equal in size to that of the solar one, seemed to be verified by this, as the extent appeared nearly of the same dimensions. The wind was at south-west.

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DEAR SIR,

Wycliffe, Oct. 23, 1782.

I TROUBLED you early in spring with an account of a rather singular phenomenon, seen here on the 27th of February, between seven and eight o'clock, viz. a *lunar Iris* with prismatic colours. Since that I have seen two more here; one July the 30th, about eleven o'clock, which lasted about a quarter of an hour, without colours; the last, the cause of my

my troubling you with this, was on Friday the 18th instant, perhaps the most extraordinary one of the kind ever seen, and of which I was myself a spectator for most of its duration, as were many in my house and neighbourhood. It was first visible about nine o'clock, and continued, though with very different degrees of brilliancy, till past two. At first, though a strongly marked bow, it was without colours; but afterwards they were very conspicuous and vivid in the same form as in the solar, though fainter; the red, green, and purple, were most distinguishable. About twelve it was the most splendid in appearance; its arc was considerably a smaller segment of a circle than a solar; its south-east limb first began to fail, and a considerable time before its final extinction; the wind was very high, nearly due west, most part of the time, accompanied with a drizzling rain. It is a singular circumstance, that three of these phenomena should have been seen in so short a time in one place, as they have been esteemed ever since the time of ARISTOTLE, who is said to have been the first observer of them, and saw only two in fifty years, and since by PLOT and THORESBY, almost the only two English authors who have spoke of them, to be exceeding rare. They seem evidently to be occasioned by a refraction in a cloud or turbid atmosphere, and in general indications of stormy and rainy weather, so bad a season as the late summer having, I believe, seldom occurred in England. THORESBY, indeed, says, the one he observed was succeeded by several days of fine serene weather.

One particular, rather singular, in the second, *viz.* of July the 30th, was its being six days after the full of the moon, and the last, though of so long a duration, was three days before the full; that of the 27th of February was exactly

exactly at the full, which used to be judged the only time they could be seen, though in the Encyclopedie there is an account that WEIDLER observed one in 1719, in the first quarter of the moon, with faint colours, and in very calm weather.

No lunar Iris, I ever heard or read of, lasted near so long as that on the 18th instant, either with or without colours.



*WH. Account of an Earthquake. By John Lloyd, Esq. in a  
Letter to Sir Joseph Banks, Bart. P. R. S.*

Read January 23, 1783.

TO SIR JOSEPH BANKS, BART. P. R. S.

DEAR SIR,

Wickwer near St. Asaph,  
Nov. 16, 1782.

UPON Saturday the 5th of October last, between eight and nine o'clock in the evening, a shock of an earthquake was felt in several parts of this principality, by many persons, though not generally. At Mold, in the county of Flint, it was distinctly perceived by a gentleman, at that time in a house quite out of the town, and seemed attended with a rumbling noise, like a carriage going over a pavement, so that if his situation had not rendered that sensation impossible, he should have ascribed it to that; and at the same time some China cups and saucers rattled very much, that were upon a table in the room with him.

At the palace, at Bangor, it was perceived by all the bishop's family at about thirty-nine minutes past eight o'clock, with the same kind of rumbling, and a double vibration. Many other persons in that neighbourhood were sensible of it.

In many places in the isle of Anglesey it was strongly felt; at Bodorgan, the seat of OWEN P. MEYRICK, esq. it was thought by the family that a carriage had driven up to the door. In answer to some enquiries made, I received the following

lowing account of an ingenious friend of mine, who is concerned in the great copper mine at Paris Mountain, and was at that time within a mile of the mine at his own house.

“ I perceived the earthquake to begin at Amlwch at 40' past eight o'clock at night, on Saturday the 5th of October. The shock was great and alarming. The house in which I was was shaken terribly, and underwent several vibrations for the continuance of near a quarter of a minute. I thought it moved from N.E. to S.W. but was not certain. It was attended with a rumbling noise, as loud as thunder, and like it just before it ceases. I have made an enquiry at several distant parts in the island to the S.W. about it, to have found out, if possible, at what rate it moved, but in vain.”

At the time it was felt in the places I have mentioned, I was at St. Asaph with some other gentlemen, looking over some parish accounts; but none of us perceived it, though it was perceived by a relation of mine, who was then alone, reading at the distance of a mile and a half from us, and in the line between Anglesey and Mold, so that I was probably further north than the shock reached. I judge every phenomenon of this kind to be interesting to the speculative observers of nature. You may probably be of the same opinion; and if you are, and should think this imperfect account of sufficient consequence, you will please to lay it before the Royal Society.

I have the honour to be, &c.



VIII. *An Account of a new Eudiometer.*  
*By Mr. Cavendish, F. R. S.*

Read January 16, 1783.

**D**R. PRIESTLEY's discovery of the method of determining the degree of phlogistication of air by means of nitrous air, has occasioned many instruments to be contrived for the more certain and commodious performance of this experiment; but that invented by the Abbé FONTANA is by much the most accurate of any hitherto published. There are many ingenious contrivances in his apparatus for obviating the smaller errors which this experiment is liable to; but the great improvement consists in this, that as the tube is long and narrow, and the orifice of the funnel not much less than the bore of the tube, and the measure is made so as to deliver its contents very quick, the air rises slowly up the tube in one continued column; so that there is time to take the tube off the funnel, and to shake it before the airs come quite in contact, by which means the diminution is much greater and much more certain than it would otherwise be. For instance, if equal measures of nitrous and common air are mixed in this manner, the bulk of the mixture will, in general, be about one measure; whereas, if the airs are suffered to remain in contact about one-fourth of a minute before they are shaken, the bulk of the mixture will be hardly less than one measure and two-tenths, and will be very different according as it is suffered to

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remain a little more or a little less time before it is shaken. In like manner, if through any fault in the apparatus, the air rises in bubbles, as in that case it is almost impossible to shake the tube soon enough, the diminution is less than it ought to be.

Another great advantage in this manner of mixing is, that thereby the mixture receives its full diminution in the short time during which it is shaken, and is not sensibly altered in bulk after that; whereas, if the airs are suffered to remain some time in contact before they are shaken, they will continue diminishing for many hours.

The reason of the abovementioned differences seems to be, that in the Abbé FONTANA's method the water is shaken briskly up and down in the tube while the airs are mixing, whereby each small portion of the nitrous air must be in contact with water, either at the instant it mixes with the common air, or at least immediately after; and it should seem, that when the airs are in contact with water during the mixing, the diminution is much greater and more certain than when there is no water ready to absorb the nitrous acid produced by the mixture. This induced me to try whether the diminution would not be still more certain and regular if one of the two kinds of air was added slowly to the other in small bubbles, while the vessel containing the latter was kept continually shaking. I was not disappointed in my expectations, as, I think, this method is really more accurate than the Abbé FONTANA's; and, moreover, in the course of my experiments I had occasion to observe a circumstance which is necessary to be attended to by those who would examine the purity of air with exactness by any kind of eudiometer, besides some others which tend very

much to explain many of the phenomena attending the mixture of common and nitrous air.

The apparatus I use is as follows. A (fig. 1.) is a cylindrical glass vessel, with brass caps at top and bottom; to the upper cap is fitted a brass cock B; the bottom cap is open, but is made to fit close into the brass socket Dd, and is fixed in it in the same manner as a bayonet is on a musquet. The socket Dd has a small hole E in its bottom, and is fastened to the board of my tub by the bent brass FfG, in such manner that *b*, the top of the cock, is about half an inch under water; consequently if the vessel A is placed in its socket, with any quantity of air in it, and the cock is then opened, the air will run out by the cock, but will do so very slowly, as it can escape no faster than the water can enter by the small hole E to supply its place.

Besides this vessel, I have three glass bottles like M (fig. 2.) each with a flat brass cap at bottom to make it stand steady, and a ring at top to suspend it by, and also some measures of different sizes such as B (fig. 3.); these are of glass with a flat brass cap at bottom and a wooden handle. In using them they are filled with the air wanted to be measured, and then set upon the brass knob C fitted upon the board of my tub below the surface of the water, which drives out some of the air, and leaves only the proper quantity. This measure is easier made, and more expeditious in using, than the Abbé FONTANA's, and, I believe, is equally accurate; but if it was not it would not signify, as I determine the exact quantity of air used by weight.

There are two different methods of proceeding which I have used; the first is to add the respirable air slowly to the nitrous; and the other, to add the nitrous air in the same manner to the respirable.

respirable. The first is what I have commonly used, and which I shall first describe. In this method a proper quantity of nitrous air is put into one of the bottles M, by means of one of the measures above described, and a proper quantity of respirable is let into the vessel A, by first filling it with this air, and then setting it on the knob C, as was done by the measure. The vessel A is then fixed in the socket, and the bottle M placed with its mouth over the cock. Then on opening the cock, the air in the vessel A runs slowly in small bubbles into the bottle M, which is kept shaking all the time by moving it backwards and forwards horizontally while the mouth still remains over the cock.

Notwithstanding the precautions used by the Abbé FONTANA in measuring the quantity of air used, I have sometimes found that method liable to very considerable errors, owing to more water sticking to the sides of the measure and tube at one time than at another: for this reason I determine the quantities of air used and the diminution, by weighing the vessels containing it under water in this manner. From one end of a balance, placed so as to hang over the tub of water, is suspended a forked wire, to each end of which fork is fixed a fine copper wire; and in trying the experiment the vessel A, with the respirable air in it, is first weighed, by suspending it from one of these copper wires, in such manner as to remain intirely under water. The bottle M, with the proper quantity of nitrous air in it, is then hung on in the same manner to the other wire, and the weight of both together found. The air is then let out of the vessel A into the bottle M, and the weight of both vessels together found again, by which the diminution of bulk which they suffer on mixing is known. Lastly, the bottle M is taken off, and the vessel A weighed again.

again by itself, which gives the quantity of respirable air used. It is needless to determine the quantity of nitrous air by weight; because, as the quantity used is always sufficient to produce the full diminution, a small difference in the quantity makes no sensible difference in the diminution \*. In this manner of determining the quantities by weight, care should be taken to proportion the lengths of the copper wires in such manner that the surface of the water in A and M shall be on

\* Mr. DE SAUSSURE also determines the quantity of air which he uses by weight; but does it by weighing the vessels containing it in air. This method is liable to some inaccuracy, as the air in the vessel is apt to be compressed by putting in the stopper; though, I believe, that, if care is taken to push in the stopper slowly, the error arising from thence is but small. It is also less expeditious than weighing them under water, as some time is necessarily lost in wiping the wet off the vessels; but, on the other hand, it requires less apparatus, which makes it fitter for a portable apparatus as Mr. DE SAUSSURE's was. If any gentleman is desirous of adapting this method of determining the quantities to the above described manner of mixing the airs, nothing more is required than to have glass stoppers fitted to the vessel A and to the bottle M.

It is needless to mention, that in both these methods no sensible error can arise from any difference in the specific gravity of the air; for the thing found by weighing the vessel is the difference of weight of the included air and of an equal bulk of water, which, as no air is less than 500 times lighter than water, is very nearly equal to the weight of a quantity of water, equal in bulk to the included air.

It must be observed, that a common balance is not convenient for weighing the vessels of air under water, without some addition to it; for the lower the vessel of air sinks under the water, the more the air is compressed, which makes the vessel heavier, and thereby causes that end of the beam to preponderate. This makes it necessary either to have the index placed below the beam, as in many assay balances; or by some other means to remove the center of gravity of the beam so much below the center of suspension as to make the balance vibrate, notwithstanding the tendency which the compressibility of the air in the vessels has to revert it.

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the same level when both have the usual quantity of air in them, as otherwise some errors will arise from the air being more compressed in one than in the other. This precaution indeed does not entirely take away the error, as the level of the water in M is not the same after the airs are mixed as it was before; but in vessels of the same size as mine, the error arising from thence can never amount to the 500th part of the whole, which is not worth regarding; and indeed if it were much greater, it would be of very little consequence, as it would be always the same in trying the same kind of air.

There are several contrivances which I use, in order to diminish the trouble of weighing the vessels; but I omit them, as the description would take up too much room.

The vessel A holds 282 grains of water, and is the quantity which I shall distinguish by the name of one measure. I have three bottles for mixing the airs in, with a measure B for the nitrous air adapted to each. The first bottle holds three measures, and the corresponding measure  $1\frac{1}{2}$ ; the second bottle holds six, and the corresponding measure  $2\frac{1}{2}$ ; and the third bottle holds 12, and the corresponding measure 5. The first bottle and measure is used in trying common air, or air not better than that; the two other in trying dephlogisticated air. The quantity of respirable air used, as was said before, is always the same, namely, one measure; consequently, in trying common air I use  $1\frac{1}{2}$  measures of nitrous air to one of common; and in trying very pure dephlogisticated air I use five measures of nitrous air to one of the dephlogisticated. I believe there is no air so much dephlogisticated as to require a greater proportion of nitrous than that. The way by which I judge whether the quantity of nitrous air used is sufficient, is by the bulk of the two airs when mixed; for if that is not less



less than one measure, that is, than the respirable air alone, it is a sign that the quantity of nitrous air is sufficient, or that it is sufficient to produce the full diminution, unless it is very impure.

Though the quantity of respirable air used will be always nearly the same, as being put in by measure; yet it will commonly be not exactly so, for which reason the observed diminution will commonly require some correction: for example, suppose that the observed diminution was 2.353 measures, and that the quantity of respirable air was found to be .985 of a measure; then the observed diminution must be increased by  $\frac{1}{1000}$  of the whole or .035, in order to have the true diminution, or that which would have been produced if the respirable air used had been exactly one measure; consequently, the true diminution is 2.388.

The method of weighing, described in p. 109. is that which I use in trying air much different in purity from common air; but in trying common air, I use a shorter method, namely, I do not weigh the vessel A at all, but only weigh the bottle M with the nitrous air in it; then mix the airs, and again weigh the same bottle with the mixture in it, and find the increase of weight. This, added to one measure, is very nearly the true diminution, whether the quantity of common air used was a little more or a little less than one measure. The reason of this is, that as the diminution produced on mixing common and nitrous air is only a little greater than the bulk of the common air, the bulk of the mixture will be very nearly the same, whether the bulk of the common air is a little greater or a little less than one measure: for example, let us first suppose, that the quantity of common air used is exactly one measure, and that the diminution of bulk on mixing is 1.08 of a measure, then

then must the increase of weight of the bottle M, on adding the common air, be .08 of a measure. Let us now suppose, that the quantity of common air used is 1.02 of a measure, then will the diminution, on adding the common air, be  $1.08 \times \frac{1.02}{1.00}$  or 1.1016 of a measure, and consequently the increase of weight of the bottle M will be  $1.1016 - 1.02$  or .0816 of a measure, which is very nearly the same as if the common air used had been exactly one measure.

In the second method of proceeding, or that in which the nitrous air is added to the respirable, I use always the same bottle, namely, that which holds three measures, and use always one measure of respirable air; and in trying common air use the same vessel A as in the first method; but for dephlogisticated air I use one that holds  $3\frac{1}{2}$  measures.

In trying the experiment I first weigh the bottle M without any air in it, and then weigh it again with the respirable air in it, which gives the quantity of respirable air used. I next put the nitrous air into the vessel A, and weigh that and the bottle M together, and then having mixed the airs, weigh them again, which gives the diminution.

From what has been just said, it appears, that in this method of proceeding I use a less quantity of nitrous air in trying the same kind of respirable air than in the former; the reason of which is, that the same quantity of nitrous air goes further in phlogisticating a given quantity of respirable air in this than in the former method, as will be shewn further on.

In both these methods I express the test of the air by the diminution which they suffer in mixing; for example, if the diminution on mixing them is two measures and  $\frac{2}{1000}$ , I call its test 2.353, and so on.

In the first method of proceeding I found, that the diminution was scarce sensibly less when I used one measure of nitrous air than when I used a much greater quantity; so that one measure is sufficient to produce the full diminution. I chuse, however, to use  $1\frac{1}{2}$ , for fear the nitrous air may be impure;  $\frac{7}{8}$ ths of a measure of nitrous air produced about  $\frac{1}{6}$ , and  $\frac{1}{4}$ ths of a measure about  $\frac{7}{8}$ ths of the full diminution.

I found also, that there was no sensible difference in the diminution whether the orifice by which the air passed out of the vessel A into the bottle M was only  $\frac{1}{3}$ th of an inch in diameter, or whether it was  $\frac{1}{2}$ th of an inch; that is, whether the air escaped in smaller or larger bubbles. The diminution was rather less when the bottle was shook gently than when briskly; but the difference between shaking it very gently and as briskly as I could was not more than  $\frac{1}{100}$ dth of a measure. But if it was not shaken at all the diminution was remarkably less, being at first only ,9; in about 3', indeed, it increased to ,93, and after being shaken for about a minute it increased to ,99; whereas, when the bottle was shaken gently, the diminution was 1,08 at first mixing, and did not increase sensibly after that time. The difference proceeding from the difference of time which the air took up in passing into the bottle was rather greater; namely, in some trials, when it took up 80'' in passing, the diminution was  $\frac{1}{100}$ dths greater than when it took up only 22'', and about  $\frac{1}{100}$ dths greater than when it took up 45'; in some other trials, however, the difference was less. It appears, therefore, that the difference arising from the difference of time which the air takes up in passing into the bottle is considerable; but, as with the same hole in the plate Dd it will take up always nearly the same time, and as it is easy adjusting the size of the hole, so as to make it take up nearly the  
time

time we desire, the error proceeding from thence is but small. The time which it took up in passing in my experiments was usually about 50''.

The difference proceeding from the difference of size of the bottle, and the nature of the water made use of is greater; for when I use the small bottle which holds three measures, and fill it with distilled water, the usual diminution in trying common air is 1.08; whereas, if I fill the bottle with water from my tub, the diminution is usually about .05 less. If I use the bottle which holds twelve measures, filled with distilled water, the diminution is about 1.15; and if I use the same bottle, filled with water from my tub, about 1.08.

The reason of this difference is, that water has a power of absorbing a small quantity of nitrous air; and the more dephlogisticated the water is, the more of this air it can absorb. If the water is of such a nature also as to froth or form bubbles on letting in the common air, the diminution is remarkably less than in other water.

The following table contains the diminution produced in trying common air in the bottle containing three measures, with several different kinds of water, and also the diminution which the same quantity of nitrous air suffered by being only shook in the same bottle, without the addition of any common air, tried by stopping the mouth of the bottle with my finger, and shaking it briskly for one minute, and afterwards for one minute more.

Diminution in trying common air.	Diminution on shaking ni- trous air for		
	one minute	two minutes.	
1.099	.118	.122	Distilled water.
1.049	.083	.088	Water from tub.
1.036	.090	.098	Pump water.
1.062	.090	.099	{ Distilled water, in which a few drops of liver of sulphur were kept for a few days.
1.045	.052	.056	{ Distilled water impregnated with nitrous air, by keeping it with about $\frac{1}{4}$ of its bulk of ni- trous air for two days, and frequently shaking it.
.897	.082	.085	{ Water fouled by oak shavings. N. B. it frothed very much.

In general, the diminution was nearly as great with rain water as distilled water; but sometimes I have found rain water froth a good deal, and then the diminution was not much greater than by the water fouled with oak shavings.

This difference in the diminution, according to the nature of the water, is a very great inconvenience, and seems to be the chief cause of uncertainty in trying the purity of air; but it is by no means peculiar to this method, as I have found as great a difference in FONTANA's method, according as I have filled the tube with different waters\*. But it shews plainly, how little all the experiments which have hitherto been made for determining the variations in the purity of the atmosphere can be relied on, as I do not know that any one before has been attentive to the nature of the water he has used, and the difference proceeding from the difference of waters is much greater than any I have yet found in the purity of air.

\* I do not find that it makes much difference in FONTANA's method whether the water is disposed to froth or not; but the advantage which it has in that respect over this method is not of much consequence, as it is easy, finding water which will not froth.

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The best way I know of obviating this inconvenience is to be careful always to use the same kind of water: that which I always use is distilled, as being most certain to be always alike. I should have used rain water, as being easier procured, if it had not been that this water is sometimes apt to froth, which I have never known distilled water do.

As I found that the power with which the distilled water I used absorbed nitrous air was greater at some times than others, which must necessarily make an error in the observation, I was in hopes that, by observing the quantity of nitrous air which the water absorbed in the same manner as in the preceding experiment, together with the heat of the water, as that also seems to affect the experiment, one might be able to correct the observed test, and thereby obviate the error which would otherwise arise from any little difference in the nature of the water employed. With this view I made the following experiment.

I purged some distilled water of its air by boiling, and kept one part of it for a week in a bottle along with some dephlogisticated air, and shook it frequently; the other part was treated in the same manner with phlogisticated air. At the end of this time I found, by a mean of three different trials, that the test of common air tried with the first of these waters was 1.139, the diminution which nitrous air suffered by being shook 2' in it in the usual manner was .285. The test of the same air tried with the last of these waters was only 1.054, and the diminution of nitrous air only .090, the heat of the water in the tub and of the distilled waters being 45°. I then raised both the water of the tub and the distilled waters to the heat of 67°, and found that the test of the same air, tried by the first water, was then 1.100, and by the latter 1.044; and that  
the

the diminution of nitrous air was .235 by the first water, and .089 by the latter.

It should seem from hence, as if the observed test ought to be corrected by subtracting  $\frac{1}{4}$ ths of the diminution which nitrous air suffers by being shaken in the water, and adding .002 for every 3° of heat above 0, as the foregoing trials will agree very well together, if they are corrected by this rule, and better than if corrected by any different rule, as will appear by the following table.

	Heat.	Diminution of nitrous air.	Observed test.	Correction for Diminution.	Heat.	Corrected test.
Former water	{ 45	.285	1.139	.114	.030	1.055
	{ 67	.235	1.100	.094	.045	1.051
Latter water	{ 45	.090	1.054	.036	.030	1.048
	{ 67	.089	1.044	.036	.045	1.053

Though in all probability this correction will diminish the error proceeding from a difference in the nature of the distilled water employed, yet I have reason to think, that it will by no means entirely take it away; for which reason I do not in general make use of it. In almost all the trials, indeed, in which I have applied the correction, it has come out very nearly the same; which seems to shew, that there was no other difference in the absorbing power of the distilled water I employed, than what proceeded from its difference of heat. The above experiment, however, shews plainly, that distilled water is capable of a very great difference in this respect independent of its heat.

In the second method of proceeding, or that in which the nitrous air is added to the respirable, I found nearly the same difference

difference in the diminution, according as the bottle was shaken briskly or gently, as in the former method: I found also nearly the same difference, or perhaps rather less, according to the nature of the water employed, only it seemed to be of not much consequence whether the water frothed or not; but there seemed to be much less difference in the diminution, according to the time which the air took up in passing into the bottle. The usual diminution on trying common air with different quantities of nitrous air, when distilled water was employed, was as follows:

Common air.	Nitrous air.	Diminution.
1. {	.6	.74
	.8	.88
	1.	.89
	1.5	.90

It appears, therefore, that  $\frac{1}{10}$ ths of a measure of nitrous is sufficient to produce very nearly the full diminution. I chuse, however, always to use one measure. It appears also, that the diminution is always much less in this method than when the common air is added to the nitrous; as in that method it was before said, that the usual diminution was 1.08. The reason of this is, that when nitrous and common air are mixed together, the nitrous air is robbed of part of its phlogiston, and is thereby turned into phlogisticated nitrous acid, and is absorbed by the water in that state, and besides that, the common air is phlogisticated, and thereby diminished: so that the whole diminution on mixing is equal to the bulk of nitrous air, which is turned into acid, added to the diminution which the common air suffers by being phlogisticated. Now it appears, that when a small quantity of nitrous air comes in contact with a large quantity



quantity of common air, it is more completely deprived of its phlogiston, and is absorbed by the water in a more dephlogisticated state than when a small quantity of common air comes in contact with a large quantity of nitrous; consequently, in the second method, where small portions of nitrous air come in contact with a large quantity of common air, the nitrous air is more deprived of its phlogiston, and therefore a less quantity of it is required to phlogistificate the common air than in the first method, where small portions of common air come in contact with a large quantity of nitrous air; so that a less quantity of the nitrous air is absorbed in the second method than in the first. As to the common air, as it is completely phlogisticated in both methods, it most likely suffers an equal diminution in both.

A clear proof that a less quantity of nitrous is required to phlogistificate a given quantity of common air in the second method than in the first, is, that if common air is mixed with a quantity of nitrous air not sufficient to completely phlogistificate it, the mixture will be more phlogisticated if the nitrous air is added slowly to the common, as in the second method, than if the common air is added to the nitrous; and if the nitrous air is added slowly to the common, without being in contact with water, the mixture will be found to be still more phlogisticated than in the second method, where the two airs are in contact with water at the time of mixing.

The following table contains the result of the experiments I have made on this subject.

First

First method.			Second method.			Nitrous air added slowly to common without being in contact with water.		
Nitrous air.	Bulk of mixture	Test.	Nitrous air.	Bulk of mixture	Test.	Nitrous air.	Bulk of mixture	Test.
.716	.856	.244	.635	.849	.137			
.474	.915	.513	.430	.867	.352	.294	.836	.337
			.280	.930	.599			

The two first sets of experiments were not tried with the apparatus above described, as that held too small a quantity, but with another upon the same principle. The last set was tried by the apparatus represented in fig. 4. where A is a bottle containing nitrous air, inverted into the tub of water DE; B is a bottle with a bent glass tube C fitted to its mouth. This bottle is filled with common air, without any water, and is first slightly warmed by the hand; the end of the glass tube is then put into the bottle of nitrous air, as in the figure; consequently, as the bottle B cools, a little nitrous air runs into it, which, by the common air in it, is deprived of its elasticity, so that more nitrous air runs in to supply its place. By this means the nitrous air is added slowly to the common without coming in contact with water, till the whole of the nitrous air has run out of the bottle A into B; then, indeed, the water runs through the glass tube into B, to supply the vacancy formed by the diminution of the common air.

It appears from the foregoing table, that a quantity of nitrous air, used in the first method, does not phlogistificate common air more than three-fourths of that quantity used in the second way does, and not so much as half that quantity used in the third way: so that we may safely conclude, that it is this circumstance of the nitrous air going further in phlogisti-

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cating common air in some circumstances than others, which is the cause that the diminution in trying the purity of air by the nitrous test is so much greater in some methods of mixing them than in others.

From what was said in p. 119. it should seem as if the second method was more exact than the first, as the error proceeding from the air employing more or less time in passing into the bottle was found to be less, and that proceeding from a difference in the water, and from the bottle being shaken more or less strongly was not greater. I, however, have found, that the trials of the same air on the same day have commonly differed more when made in this manner than in the first; for which reason, and because in trying common air the first method takes up the least time, I have commonly used that.

It should be observed, that in trying dephlogisticated air by the first method it is convenient to use different bottles, according to the different purity of the air; and the same air will appear purer, if tried by a larger bottle than by a smaller. For example, if its test, tried by the large bottle, comes out 2.54, it will appear not more than 2.44, if tried by the middle bottle; and, in like manner, if its test by the middle bottle comes out 1.11, it will appear to be about 1.08, if tried by the least bottle; for this reason it is right always to set down which bottle it is tried by.

I think I may confidently assert, that either of the above methods are considerably more accurate than FONTANA's, supposing the experiment to be made exactly in his manner, that is, determining the quantities by measure. But, in order to judge which method of mixing the airs is most exact, it was necessary to determine the quantities in his method also by weight, as otherwise it would be uncertain whether my method of  
mixing

mixing the airs is really better than his, or whether the apparent greater exactness proceeds only from the superiority of weighing above measuring: for this reason I made some experiments in which common, and nitrous air were mixed in his manner, except that I used only one measure of each, as Dr. INGEN-HOUZ did, and that the nitrous air was put up first, the true diminution being determined by weight, by first weighing the tube under water with the nitrous air in it, and then adding the common air, and weighing the tube again under water. It was unnecessary, for the reasons given in p. 110. and 112. to determine the quantity of either the nitrous or common air by weight. My reason for this variation was, that it afforded a much easier method of determining the quantities by weight, was less trouble, and, I believe, must be at least as exact: for I have always found, that the experiments made with the Abbé FONTANA's apparatus, in which I used only one measure of each air, agreed better together than those in which I used two of common, and added the nitrous air by one at a time; and I imagine it can be of no signification whether the nitrous or common air is put in first, as I cannot perceive the diminution to be sensibly greater in one of these ways than the other\*.

\* It is not extraordinary, that in this method the diminution is just the same whether the common or nitrous air is put up first, notwithstanding that in mine it is very different; since, in this method the two airs mix in the same manner whichever is put up first: whereas in mine, the manner in which they mix is very different in those two cases; as in one, small portions of common air come in contact with large portions of the nitrous; and in the other, small portions of nitrous air come in contact with large portions of common air.

From the result of these experiments I am persuaded, that my method of mixing the airs is really rather more accurate than FONTANA's, as in trying the same bottle of air six or seven times in my method the different trials would not often differ more than  $\frac{1}{1000}$ th part, and very seldom more than  $\frac{1}{500}$ th; whereas in his there would commonly be a difference of  $\frac{1}{1000}$ th, and frequently near twice that quantity, though I endeavoured to be as regular as I could in my manner of trying the experiment. My method also certainly requires less dexterity in the operator than his.

It is of much importance towards forming a right judgement of the degree of accuracy to be expected in the nitrous test, to know how much it is affected by a difference in the nitrous air employed. Now it must be observed, that nitrous air may differ in two respects; first, it may vary in purity, that is, in being more or less mixed with phlogisticated or other air; and, secondly, it is possible, that out of two parcels equally pure one may contain more phlogiston than the other. If it differs in the second respect, it will evidently cause an error in the test, in whatever proportion it is mixed with the respirable air; but if it differs only in the first respect, it will hardly cause any sensible error, unless it is more than usually impure, provided care is taken to use such a quantity as is sufficient to produce the full diminution. This has been observed by the Abbé FONTANA, and agrees with my own experiments; for the test of common air tried in my usual method, with some nitrous air which had been debased by the mixture of common air, came out only 18 thousandths less than when tried with air of the best quality, though this air was so much debased that the diminution, on mixing two parts of this with five of common, was one-sixth part less than when good nitrous air was

was employed; which shews, that the error proceeding from the difference of purity of the nitrous air is much less when it is used in the full quantity than in a smaller proportion; and also shews, that if it is used in the full quantity it can hardly cause any sensible error, unless it is more impure than usual. One does not easily see, indeed, why it should cause any error; for no reason appears why the mixture of phlogisticated or other air, not absorbable by water, and not affected by respirable air, should prevent the nitrous air from diminishing and being diminished by the respirable air in just the same manner that it would otherwise be. It must be observed, however, that if the nitrous air is mixed with fixed air, it will cause an error, as part of the fixed air will be absorbed by the water while the test is trying; for which reason care should be taken that the nitrous air should not be much mixed with this substance, which it will hardly be, unless either the metal it is procured from is covered with rust; or unless the water in which it is received contains much calcareous earth suspended by fixed air; as in that case, if any of the nitrous acid comes over with the air, it will dissolve the calcareous earth, and separate some fixed air.

In order to see whether it is possible for nitrous air to differ in the second respect, I procured some from quicksilver, copper, brass, and iron, and observed the test of the same parcel of common air with them, on the same day, making four trials with each, when the difference between the tests tried with the three first kinds of air was not greater than might proceed from the error of the experiment; but those tried with the air from iron were  $\frac{1}{8}$ th greater than the rest. I then took the test of some more common air with them in the same manner, only using four parts of common to one of nitrous air, when

when the tests tried with the air from iron came out smaller than the rest by not less than  $\frac{1}{1000}$ ths. It should seem, therefore, from these experiments, that the nitrous air procured from iron, besides being much more impure than the others, differs from them also in the second respect; that is, that the pure nitrous air in it contains rather less phlogiston than that in the others: whence it happens, that a greater quantity is necessary to phlogificate a given portion of common air, and consequently that the diminution is greater when a sufficient quantity of it is used, though with a less proportion the diminution is much less than with other nitrous air, on account of its greater impurity. As for the air procured from the three other substances, I cannot be sure that there is any difference between them. The nitrous air I always use is made from copper, as it is procured with less trouble than from quicksilver, and I have no reason to think it more likely to vary in its quality.

During the last half of the year 1781, I tried the air of near 60 different days, in order to find whether it was sensibly more phlogificated at one time than another; but found no difference that I could be sure of, though the wind and weather on those days were very various; some of them being very fair and clear, others very wet, and others very foggy.

My way was to fill bottles with glass stoppers every now and then with air from without doors, and preserve them stopped and inverted into water, till I had got seven or eight, and then take their test; and whenever I observed their test, I filled two bottles, one of which was tried that day, and the other was kept till the next time of trying, in order to see how nearly the test of the same air, tried on different days, would agree. The experiment was always made with distilled water, and care was

always taken to observe the diminution which nitrous air suffered by being shaken in the water, as mentioned in p. 115. The heat of the water in the tub also was commonly set down. Most of the bottles were tried only in the first method; but some of them were also tried by the second, and by the method just described in the manner of FONTANA.

The result was, that the test of the different bottles tried on the same day never differed more than .013, and in general not more than half that quantity. The test, indeed, of those tried on different days differed rather more; for taking a mean between the tests of the bottles tried on the same day, there were two of those means which differed .025 from each other; but, except those two, there were none which differed more than .013. Though this difference is but small, yet as each of these means is the mean of seven or eight trials, it is greater than can be expected to proceed from the usual errors of the experiment. This difference also is not much diminished by correcting the observations on account of the heat and absorbing power of the water, according to the rule in p. 118. This might incline one to think, that the parcels of air examined on some of those days of trial were really more dephlogisticated than the rest; but yet, I believe, that they were not: for whenever there was any considerable difference between the means of two successive days of trial, there was nearly the same difference between the tests of the two bottles of the very same air tried on those two days. For example, the mean of the trials on July 7. was .016 less than that of those on the 15th of the same month; but then the test of the air caught and tried on the 7th was equally less than that of the air of the same day tried on the 15th; which shews, that this difference between the means of those two days was not owing to



to the parcels of air tried on the former day being really more dephlogisticated than those tried on the latter, but only to some unperceived difference in the manner of trying the experiment; or else to some unknown difference in the nature of the water or nitrous air employed. A circumstance which seems to shew that it was owing to the first of these two causes is, that it frequently happened, that on those days in which the tests taken in the first method came out greater than usual, those taken in FONTANA's manner, or in the second method, did not do so; the trials, however, made in these two methods were too few to determine any thing with certainty. On the whole there is great reason to think, that the air was in reality not sensibly more dephlogisticated on any one of the sixty days on which I tried it than the rest.

The highest test I ever observed was 1.100, the lowest 1.068, the mean 1.082.

I would by all means recommend it to those who desire to compare the air of different places and seasons, to fill bottles with the air of those places, and to try them at the same time and place, rather than to try them at the time they were filled, as all the errors to which this experiment is liable, as well those which proceed from small differences in the manner of trying the experiment, as those which proceed from a difference in the nature of the water and nitrous air, will commonly be much less when the different parcels of air are tried at the same time and place than at different ones; provided only, that air can be kept in this manner a sufficient time without being injured, which I believe it may, if the bottles are pretty large, and care is taken that they, as well as the water used in filling them with air, are perfectly clean. I have tried air kept in the abovementioned manner for upwards of three-quarters of a year

year in bottles holding about a pint, which I have no reason to think was at all injured; but then I have tried some kept not more than one-third part of that time which seemed to have been a little impaired, though I do not know what it could be owing to, unless it was that the bottles were smaller, namely, holding less than one-fourth of a pint, and that in all of them, except two, which were smaller than the rest, the stopper which, however, fitted in very tight, was tied down by a piece of bladder.

I made some experiments also to try whether the air was sensibly more dephlogisticated at one time of the day than another, but could not find any difference. I also made several trials with a view to examine whether there was any difference between the air of London and the country, by filling bottles with air on the same day, and nearly at the same hour, at Marlborough-street and at Kensington. The result was, that sometimes the air of London appeared rather the purest, and sometimes that of Kensington; but the difference was never more than might proceed from the error of the experiment; and by taking a mean of all, there did not appear to be any difference between them. The number of days compared was 20, and a great part of them taken in winter, when there are a greater number of fires, and on days when there was very little wind to blow away the smoke.

It is very much to be wished, that those gentlemen who make experiments on factitious airs, and have occasion to ascertain their purity by the nitrous test, would reduce their observations to one common scale, as the different instruments employed for that purpose differ so much, that at present it is almost impossible to compare the observations of one person with those of another. This may be done, as there seems to be so very little difference in the purity of common air at dif-

ferent times and places, by assuming common air and perfectly phlogisticated air as fixed points. Thus, if the test of any air is found to be the same as that of a mixture of equal parts of common and phlogisticated air, I would say, that it was half as good as common air; or, for shortness, I would say, that its standard was  $\frac{1}{2}$ : and, in general, if its test was the same as that of a mixture of one part of common air and  $x$  of phlogisticated air, I would say, that its standard was  $\frac{1}{1+x}$ . In like manner, if one part of this air would bear being mixed with  $x$  of phlogisticated air, in order to make its test the same as that of common air, I would say, that it was  $1+x$  times as good as common air, or that its standard was  $1+x$ ; consequently, if common air, as Mr. SCHEELÉ and LA VOISIER suppose, consists of a mixture of dephlogisticated and phlogisticated air, the standard of any air is in proportion to the quantity of pure dephlogisticated air in it. In order to find what test on the Eudiometer answers to different standards below that of common air, all which is wanted is to mix common and perfectly phlogisticated air in different proportions, and to take the test of those mixtures; but in standards above that of common air, it is necessary to procure some good dephlogisticated air, and to find its standard by trying what proportion of phlogisticated air it must be mixed with, in order to have the same test as common air, and then to mix this dephlogisticated air with different proportions of phlogisticated air, and find the test of those mixtures\*.

On

\* The rule for computing the standard of any mixture of dephlogisticated and phlogisticated air is as follows. Suppose that the test of a mixture of  $D$  parts of dephlogisticated air with  $B$  of phlogisticated air is the same as that of common air,

On this principle I found the standard answering to different tests on both my Eudiometers, and also on FONTANA's, to be as follows :

Standard.	Test by first method.	Test by second method.	Test by FONTANA abridged.				Total dimin.
			1	2	3	4	
4.8	5.02 - -	3.62	.73	.44	.13	1.02	3.98
3.61	3.72 - -	2.70	.75	.49	1.00	- -	3.
2.39	2.55 by large bottle	1.87	.76	.96	1.92	- -	2.08
0.71	2.45 by middle bottle						
1.00	1.11 by middle bottle	.89	1.00	- -	- -	- -	1.00
1.00	1.08 by least bottle						
.76	.81 - -	.69	1.23	- -	- -	- -	.77
.5	.57 - -	.51	1.45	- -	- -	- -	.55
.13	.32 - -	.31	1.66	- -	- -	- -	.34
2.00	.07 - -	.08	1.94	- -	- -	- -	.06

Standard.	Test by FONTANA's method.							Total dimin.
	1	2	3	4	5	6	7	
4.8	1.75	1.43	1.11	.78	.46	.21	1.18	7.82
3.61	1.75	1.46	1.17	.89	1.16	2.13	- -	5.87
2.39	1.76	1.50	1.25	2.06	- -	- -	- -	3.94
1.	1.81	2.12	3.12	- -	- -	- -	- -	1.88
.75	1.82	2.54	- -	- -	- -	- -	- -	1.46
.5	1.98	2.94	- -	- -	- -	- -	- -	1.06
.25	2.42	3.39	- -	- -	- -	- -	- -	.61
.0	2.91	- -	- -	- -	- -	- -	- -	.09

The phlogificated air used in these experiments was procured by means of liver of sulphur.

air, then is the standard of the dephlogificated air  $\frac{D+P}{D}$ . Let now  $\delta$  parts of this dephlogificated air be mixed with  $\phi$  parts of phlogificated air, the standard of the mixture will be  $\frac{D+P}{D} \times \frac{\delta}{\delta+\phi}$ .

The trials, called FONTANA abridged, were made in the Abbé FONTANA's manner, except that only one measure of respirable air was used, the nitrous air being added by one measure at a time as usual. The column marked 1 at top is the bulk of the mixture after one measure of nitrous air was added; that marked 2, its bulk after two measures were added, and so on.

It must be observed, that in these experiments a considerable diminution took place in taking the test of the unmixed phlogisticated air, or that whose standard is marked 0 in the table; but, notwithstanding this, the air, as far as I could perceive, was perfectly phlogisticated, the diminution being caused merely by the absorption of the nitrous air by the water. What shews this to be the case is, that if common and nitrous air are mixed in such proportions as that the nitrous should be predominant, so as to be considerably diminished by the mixture of common air, this mixture will produce as great a diminution with nitrous air as the phlogisticated air used in these experiments; and if plain nitrous air is added to nitrous air, the diminution is still greater. This shews, that a considerable diminution is produced by mixing perfectly phlogisticated air with nitrous air, and also that air may be perfectly phlogisticated by liver of sulphur.

These experiments also shew the necessity of using such a quantity of nitrous air as is sufficient to produce the full diminution, in order to form a proper estimate of the goodness of air; for if the quantity of nitrous air is much less than that, the air you try will appear very little better than air of a much inferior quality. For example, if in taking the test of very good dephlogisticated air, only an equal bulk of nitrous air is used,

AT

used, it will appear very little better than a mixture of equal parts of this and phlogificated air; and if twice that quantity of nitrous air is used, it will appear very little better than a mixture of three parts of this air with one of phlogificated. Another great advantage of using the full quantity of nitrous air is, that thereby the error arising from any difference in its purity is very much diminished.

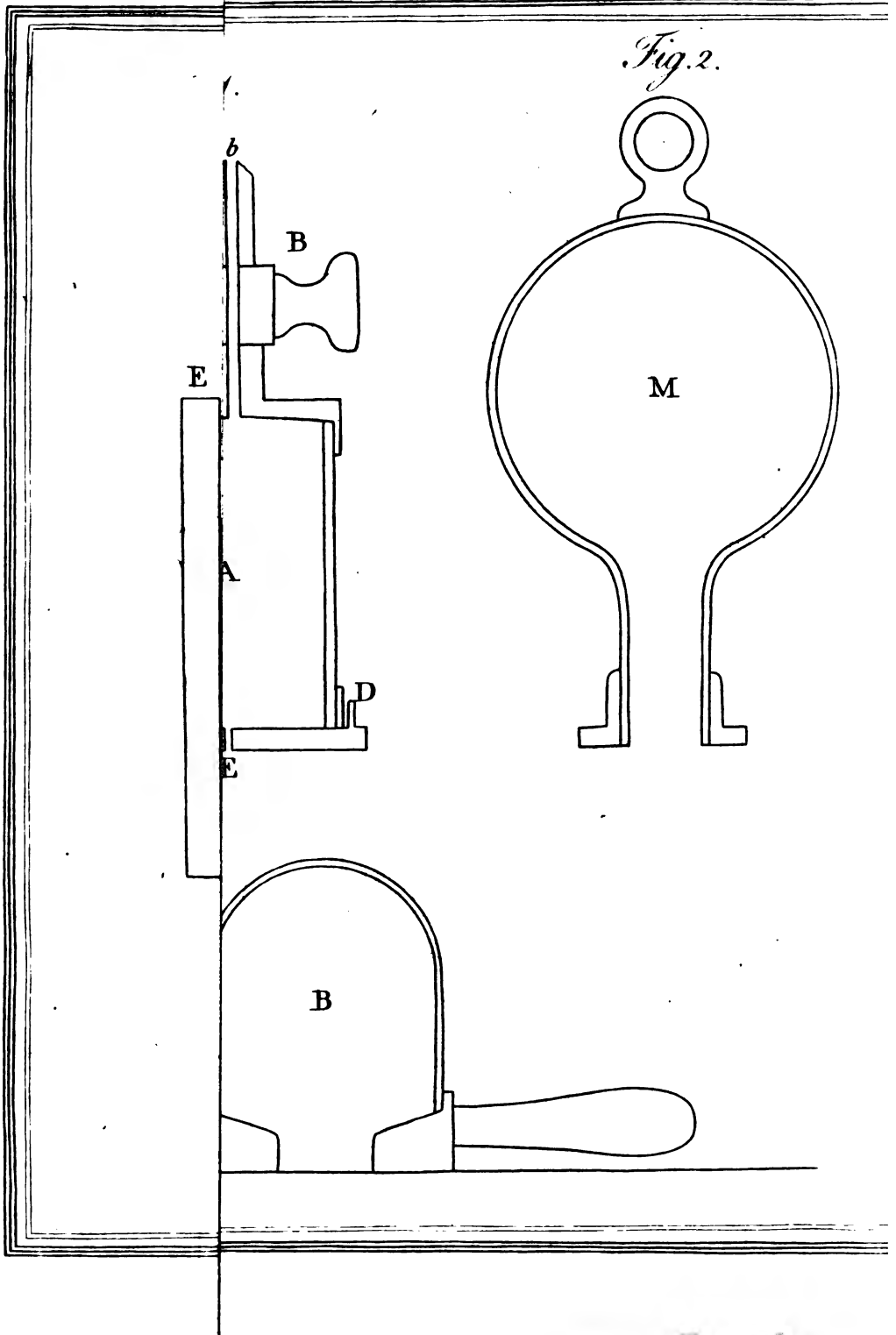
Perfectly phlogificated air may be conveniently procured by putting some solution of liver of sulphur into a bottle of air well stopped, and shaking it frequently till the air is no longer diminished, which, unless it is shaken very frequently, will take up some days. Care must be taken, however, to loosen the stopper now and then, so as to let in air to supply the place of the diminished air. In order to know when the air is as much diminished as it can be, the best way is, when the air is supposed to be nearly phlogificated, to place the bottle with its mouth under water, still keeping it stopped, and to loosen the stopper now and then, while under water, so as to let in water to supply the place of the diminished air, by which means the alteration of weight of the bottle shews whether the air is diminished or not. If the solution of liver of sulphur is made by boiling together fixed alkali, lime, and flowers of sulphur, which is the most convenient way of procuring it, the air phlogificated by it will be perfectly free from fixed air: whether it will be so if the liver of sulphur is made without lime, I am not sure.

A still more convenient way, however, of procuring phlogificated air is by a mixture of iron filings and sulphur; and, as far as I can perceive, the air procured this way is as completely phlogificated as that prepared by liver of sulphur.

Where:

Where the impurities mixed with the air have any considerable smell, our sense of smelling may be able to discover them, though the quantity is vastly too small to phlogistificate the air in such a degree as to be perceived by the nitrous test, even though those impurities impart their phlogiston to the air very freely. For instance, the great and instantaneous power of nitrous air in phlogistificating common air is well known; and yet ten ounce measures of nitrous air, mixed with the air of a room upwards of twelve feet each way, is sufficient to communicate a strong smell to it, though its effect in phlogistificating the air must be utterly insensible to the nicest Eudiometer; for that quantity of nitrous air is not more than the  $\frac{1}{140000}$ th part of the air of the room, and therefore can hardly alter its test by more than  $\frac{1}{140000}$  or  $\frac{1}{17500}$ th part. Liver of sulphur also phlogistificates the air very freely, and yet the air of a room will acquire a very strong smell from a quantity of it vastly too small to phlogistificate it in any sensible degree. In like manner it is certain, that putrifying animal and vegetable substances, paint mixed with oil, and flowers, have a great tendency to phlogistificate the air; and yet it has been found, that the air of an house of office, of a fresh painted room, and of a room in which such a number of flowers were kept as to be very disagreeable to many persons, was not sensibly more phlogistificated than common air. There is no reason to suppose from these instances, either that these substances have not much tendency to phlogistificate the air, or that nitrous air is not a true test of its phlogistification, as both these points have been sufficiently proved by experiment; it only shews, that our sense of smelling can, in many cases, perceive infinitely smaller alterations in the purity of the air than can be perceived

*Fig. 2.*







ceived by the nitrous test, and that in most rooms the air is so frequently changed, that a considerable quantity of phlogisticating materials may be kept in them without sensibly impairing the air. But it must be observed, that the nitrous test shews the degree of phlogistication of air, and that only; whereas our sense of smelling cannot be considered as any test of its phlogistication, as there are many ways of phlogisticating air without imparting much smell to it; and, I believe, there are many strong smelling substances which do not sensibly phlogisticate it.

IX. *Experiments upon the Resistance of the Air.* By Richard Lovell Edgworth, Esq. F. R. S. In a Letter to Sir Joseph Banks, Bart. P. R. S.

Read January 16, 1782.

DEAR SIR,

Edgworth's Town, Ireland,  
May 2, 1782.

THE last time I had the pleasure of seeing you, the conversation turned upon the resistance of the air, and upon a singular experiment related by Mr. ROBINS in his Treatise upon Gunnery. I have since repeated his experiment, and tried some others, which I beg you to lay before the Royal Society, if you think them worthy their attention.

I have the honour to be, &c.

MANY experiments have been tried to ascertain the force and velocity of the wind, with a view to the construction and management of different engines, and more particularly for the purposes of navigation. Several machines, which have been employed in these enquiries, are described in the Transactions of the Royal Society, and in the Memoirs of Foreign Academies; but the most accurate which I have seen was invented by the late Sir CHARLES KNOWLES; and from a number of experiments made with it, he had constructed tables, shewing at one view the force of the wind upon each sail of a ship at every degree of velocity from one to ninety miles an hour.

But

But these calculations, and many more of a similar nature, that are to be met with in BELIDOR'S *Architecture Hydraulique*, and other books, are founded upon a supposition that the effect of the wind is directly as the surface upon which it acts. If, for instance, its force be estimated as one upon one square yard, its force upon two square yards should be estimated as two, upon three square yards as three, &c.; but in fact this proportion is not to be depended upon, nor must the resistance of surfaces be estimated merely by their extent; but several other circumstances must be taken into consideration.

No figures can resemble each other more than a parallelogram and a square having the same superficial contents, as they are both bounded by four straight lines meeting at right angles, yet they oppose different degrees of resistance to the air.

If two similar cards, for instance, are placed opposite the wind, one upon its end, and the other on its side, and both inclined to the same angle, the wind will have the greater effect upon the card that is placed end-ways.

To determine the difference of resistance between these two surfaces, and to ascertain the effect of other figures moving through the air, I tried the following experiments. The two first are to be found in Mr. ROBINS'S *Treatise upon Gunnery*; but I thought it proper to repeat them, that they might be more readily compared with others made with the same apparatus, especially as Mr. ROBINS made use of a machine constructed upon a smaller scale than mine, and turning upon friction wheels, which are not proper for machines of this nature, nor indeed for any purpose, where an uniform motion is required.

Having fastened a strong joist of wood from one side of a large room to the other, so as to form a kind of bridge at some distance from the floor, I erected a perpendicular shaft or roller, which turned freely in brass sockets fixed into the floor and bridge upon pivots of hardened steel one-sixteenth of an inch in diameter. On each side of this roller was extended an arm of deal, feather-edged, and supported by stays of the same material, feathered in the same manner, to oppose as little surface as possible to the air when in motion.

Round the upper part of this roller was wound a string of cat-gut, which, passing over pulleys properly disposed, was fastened to a scale that descended into the well of an adjoining stair-case.

The extremity of these arms described a space of more than forty feet in every revolution, the weight descending in the same time only six inches. The time in all the following experiments was the same; and, as each revolution was performed in four seconds, the velocity of the end of the arm on which the surface was fixed, was at the rate of about seven miles an hour.

The first figure that I tried was a parallelogram of tin, nine inches long, and four inches wide. Its longest side was placed parallel to the floor, at the extremity of one of the arms. Its shortest sides were inclined to an angle of forty-five degrees from the perpendicular, and in this situation it was carried round with its surface against the air.

After suffering it to revolve until I was satisfied that its motion was become uniform, I put as much weight into the scale as moved it with a velocity of five turns in twenty seconds. I then changed the situation of the parallelogram, placing its shortest sides parallel to the floor, and inclined to the same

angle as before. I now found, that more weight was required to produce the same velocity, though the quantity of surface was the same as in the preceding experiment. The weight necessary to put the machine alone in motion, with the velocity above mentioned, was two pounds and a half. When it carried the parallelogram with one of its shortest sides downwards, it required four pounds and a half additional weight; and when the parallelogram was reversed, another half pound was barely sufficient to give it the same velocity.

• The difference, therefore, occasioned by placing the same parallelogram with its longer or shorter sides inclined from the direction of its motion was equal to one-tenth of the greatest resistance.

It has been observed, that in these two experiments the mean velocity of the plane was not the same, as its extremity extended farther from the center of the machine in one than in the other. This is strictly true; but the size of the parallelogram bore so small a proportion to the length of the radius to which it was fastened, that the error arising from this circumstance is scarcely perceptible, and the advantage being in favour of that which required the least weight, I did not think it necessary to bring it into account.

Having formed a general idea of the reason of the difference in these experiments, it occurred to me, that there would be a greater disproportion between the resistance of some other figures, which Mr. ROBINS had not tried; and having put a rhomboid, in the form of a lozenge, nine inches long, and four broad, in the place of the parallelogram, the difference was increased from one-tenth to one-seventh of the weight employed to give them the required velocity.

Pursuing the same reasoning that led me to the last experiment, it occurred to me, that even against figures of exactly the same shape, the resistance of the air, when the dimensions of the figures were enlarged, would not be increased in the same proportion as the size of the planes, but in a much higher ratio; and that, by bending the planes as a sail, the resistance would be still farther increased, though the section of air, that would be intercepted by the planes, must by these means be considerably lessened.

The result far surpassed my expectations. A square of tin, containing sixteen square inches, placed perpendicularly, was resisted as two and a half. A square, containing sixty-four inches, or four times the former quantity, instead of meeting with a resistance as ten or four times the former resistance, required no less than fourteen pounds to give it the same velocity.

Four-tenths (or nearly half as much again) was an increase of resistance that made me suspect some error in the experiment; but having repeated it several times with great care, and having examined all the parts of the machine, I was satisfied that I had made no mistake.

I now placed the parallelogram of nine inches long upon the arms of the machine, with its shortest sides parallel to the horizon, bending it to such an arch that its chord measured eight inches, and inclining it to an angle of forty-five degrees. And though the section of air that it intercepted was by these means diminished one-ninth, yet the resistance was increased from five to five and a half. And when the parallelogram was bent yet farther, and its chord contracted almost to seven inches, the resistance was increased to five and three-quarters.

I mention these numbers in gross to avoid confusion; but in the

the table at the end of this paper, the measures and weights are set down exactly.

Dr. HOOK, whose name must be respected by every experimental philosopher, was aware, that although he thought he could demonstrate that flat sails were preferable to such as were curved and hollowed by the wind, yet until proper experiments had been tried, nothing could be positively determined.

He says somewhere in his posthumous works, " That he " was surprised at the obstinacy of seamen, in continuing, " after what appeared the clearest demonstration to the contrary, to prefer bellying or bunting sails to such as were " hauled taught; but that he would, at some future time, " add the test of experiment to mathematical investigation." He reasoned upon a supposition, that the air in motion followed the same laws as light; and that it was reflected from surfaces with the angle of reflection equal to the angle of incidence, which is not the case, as it never makes an angle with the plane, but is always reflected in curves. MONS. PARENT, and other mathematicians, have fallen into the same mistake. No demonstration of this sort was more commonly known or received among practical mechanics, than that the best angle for the sails of a wind-mill, at the beginning of their motion, was an angle of forty-five degrees; and that the maximum of an under-shot water-wheel was when it moved with one-third of the velocity of the water: but Mr. SMEATON, in an excellent paper in the Philosophical Transactions, has refuted this opinion by the clearest experiments.

I had intended to diversify these experiments, and to extend them to a more interesting subject of enquiry, to determine the best shape of sails, and the angle to which they should be set, to obtain the greatest progressive effect with the least leeway;



way; but, as a more complicated apparatus than I could at present procure is necessary for this purpose, I determined to offer you the slight progress I have made, in hopes that some gentleman, more conversant and more interested than myself in these inquiries, may pursue them with success and advantage to the public. I shall only remark, that the general cause of the different resistance of the air upon surfaces of different shapes, is the stagnation of that fluid near the middle of the plane upon which it strikes. The shape and size of the portion thus stagnated, differs from the shape and angle of the plane. The elasticity of the air permits the parts in motion to compress those which are first stopped or retarded by the plane, and forms, as it were, a new surface of a different shape, for the reception of those particles which succeed. With the assistance of a good solar microscope the curves of the air striking against different surfaces may be delineated, and when the general facts are once clearly ascertained, mathematicians will have an ample field for curious and useful speculation.

**T A B L E.**

T A B L E.

	Turns.	Time.	Weight.
Machine alone - - -	5	4	2 8
With a parallelogram of nine inches long and four broad, one of its longest sides, parallel to the horizon and the parallelo- gram, inclined to an angle of $45^{\circ}$ , -	5	4	7 0
Ditto, with one of its shortest sides down- wards, - - -	5	4	7 9
With a lozenge nine inches long and four broad, with its longest side parallel to the horizon, - - -	5	4	5 8
Ditto reversed, - - -	5	4	6 0
With a square piece of tin, four inches by four inches, - - -	5	4	5 0
Ditto, eight inches by eight inches,	5	4	16 6
With the former parallelogram, placed with one of its shortest sides downwards, inclined to an angle of $45^{\circ}$ , and bent into an arch whose chord was eight inches long, - - -	5	4	8 0
Ditto bent to an arch, the chord of which was seven inches and a quarter, -	5	4	8 5



X. *An Answer to the Objections stated by M. De la Lande, in the Memoirs of the French Academy for the Year 1776, against the Solar Spots being Excavations in the luminous Matter of the Sun, together with a short Examination of the Views entertained by him upon that Subject. By Alexander Wilson, M. D. Professor of Practical Astronomy in the University of Glasgow; communicated by Nevil Maskelyne, D.D. F. R. S. and Astronomer Royal.*

Read January 16, 1783.

TO THE REV. DR. MASKELYNE.

DEAR SIR,

Glasgow College  
April 20, 1782.

**H**AVING lately seen a paper on the Solar Spots by M. DE LA LANDE, in the French Memoires for 1776, published in 1779, I found such mention made of mine upon that subject, as appears to me to require an answer; not only in justice to myself, but to that honourable Society who admitted an account of my discovery into their Transactions, and to philosophy in general, whose cause it becomes a duty upon her followers to vindicate from such errors as seem likely to prevail.

The universal circulation of the volumes of the French Academy makes me the more solicitous to be heard in answer; and to have that answer conveyed to the public through the channel  
of

of the Philosophical Transactions, as being the only one where an appeal can be made equally general and extensive.

It has been my aim to draw up the paper, which at this time I take the liberty of transmitting, with that freedom which the important interest of truth demands, and to observe at the same time those rules of decorum, of which too it becomes philosophers, of all other men, to set the example.

Give me leave, therefore, to request, as a very particular favour, that you will be so good as to communicate the inclosed to your colleagues in the Council of the Royal Society; as also this letter, in which I wish to express the most unreserved deference to your and their opinions and determination.

I am, &c.

MAC FARLANE Observatory, April 18, 1782.

IN the first part of my paper, published in the Philosophical Transactions for 1774, I have explained how, from the lucky accident of seeing the great solar spot of November 1769, in a certain critical situation upon the disk, its real nature was obtruded on my thoughts by a train of appearances the most obvious and unequivocal. The reader may there also see how, from phenomena perfectly similar in spots of the usual size, I was led to a general conclusion, and to believe that all spots, small as well as great, which consist of a dark nucleus, and surrounding umbra, are excavations in the luminous matter of the sun.

Before this time no opinions had been entertained of any third dimension belonging to the spots, but what agreed to the conception of their being something material extending more

or less above the common level of the sun's spherical surface. This idea, though so very prevalent, seems however to have originated rather from pre-conceived theoretical notions of the nature and constitution of that vast body, than from any phenomena of a constant and marked kind observable in the spots themselves.

The views which I have offered upon this subject disclaim, I believe, for the first time all alliance with hypothesis, and seek after a permanence in the strong holds of no imaginary system. They make a direct appeal to our senses, upon the testimony of which alone their merits must finally rest.

As for myself, situations will sometimes occur where it would be as inconsistent with the spirit of the most guarded philosophy to harbour doubts, as on other occasions to be weakly credulous. When in the year 1769, I first mentioned the discovery in the London Chronicle, and even at the time of sending a full account of it to the Royal Society, I considered it as too early to offer my sentiments in a manner intirely decided. But in the lapse of eight more years the subsequent observations have all conspired to strengthen, and even to perfect, my convictions. Now, therefore, I have no hesitation in pledging any credit which may belong to me, as an astronomical observer, to the present and to future times, for the reality of that discovery which is laid down in the first part of the paper above mentioned.

But though I maintain that any person, with a good telescope, and with good eyes practised in observing, cannot fail of beholding frequently in the sun the phenomena I have pointed out, it yet may require the more palpable dimensions of some future great spots, in order to satisfy people less accustomed to examine and to judge of objects by means of glasses. This is

the more to be expected, as I find, that even from amongst those the most profoundly skilled, a demand has lately been made upon me to uphold what I have advanced, and to remove what seem to be stated as unfurmountable objections.

Having some little time ago seen the Memoirs of the French Academy for the year 1776, published in 1779, I there find my paper on the Solar Spots has come under the notice of a member of that illustrious body, whose name is justly held in great esteem by all astronomers, and to whom astronomy itself has many obligations. The author alluded to is M. DE LA LANDE. Though I should have been much flattered to have found my views supported by an authority so truly respectable, yet, even in his endeavours to oppose me, I honour him as a philosopher who has taken so much pains to vindicate what he doubtless believes to be juster opinions. In the most perfect confidence of a generous indulgence on his part, and with equal attachments to philosophy and to truth, my present intention is very freely to offer what arguments occur to me in favour of the solar spots being such as I have described.

First of all, it has been urged, as an objection of great weight, that the absence of the umbra on one side, when spots are near the limb, as so fully explained in my paper, is not constant. As to the fact, the reader may there see, that I was myself sufficiently aware of it, having stated three cases from my own observations, when I did not perceive this change to take place. The rev. FRANCIS WOLLASTON, LL.B. F. R. S. is the only person who (in the Philosophical Transactions) has bestowed any remarks on my publication; and though he with great candour acknowledges that, generally, the umbra changes in the manner I have determined, yet he expresses a difficulty as

to my conclusions, on account of this circumstance not obtaining universally.

Under similar expressions M. DE LA LANDE, in the Memoire before me, produces from his own observations, which appear to have been long continued, only three cases of the same kind, and from the ancient observations of Mess. CASSINI and DE LA HIRE, four more. In regard to these last, I am not sure if such obsolete ones ought to be referred to in a question of the present kind. Those excellent observers, entertaining no thoughts that any thing of moment depended upon a nice attention to the form of the spots, might easily overlook less obvious circumstances, especially when they were found near the limb. We may add further, that, even when so situated, they retain the umbra at both ends, and that whole side of it which lies farthest from the center of the disk and these parts in the aggregate, they might sometimes mistake for the umbra as not deficient in any particular place. That they did not affect what must have appeared to them a needless refinement in accuracy may be collected from the following expression of M. DE LA HIRE, found in the Mem. Acad. 1704, p. 10. As to this spot, says he, “ Je ne donne point ici, les figures differentes sous “ lesquelles cette tache a paru;” and, among other reasons for this, he gives the following: “ Car il me semble qu'on ne “ peut pas tirer d'utilité de ces sortes de figures qui changent “ continuellement.”

But even admitting the anomaly we at present consider to be much more frequent than can be contended for, still such cases can only be brought as so many exceptions from a certain *general law*, or uniformity of appearance, from which the condition of by far the greater number of spots is most undeniably deduced. The utmost, therefore, that can hence be alledged

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is, that some few spots differ from all the rest, or from the multitude, and are not like these excavations in the sun. Such cases or exceptions will not surely warrant the conclusion, that no spot can be an excavation. This would be to reverse all the rules of a just induction, by opposing to an irrefragable general argument, the force of one extremely limited and feeble.

But notwithstanding these few instances where the umbra is not found to change, when we consider how perfectly all spots resemble one another in their most striking features, there naturally arises some presumption for all under that description we have given partaking of one common nature; and for this only difference in the phenomena depending upon something which does not necessarily imply a complete generical distinction.

It comes therefore to be inquired, how far spots, which when near the middle of the disk appear equal and similar in all things, may yet differ from one another considered as excavations, or as possessing the third dimension of depth, and how far the peculiar circumstances by which they may disagree can contribute to make some resist this change of the umbra, when near the limb, much more than others.

In order to this, suppose two spots which occupy a space upon the sun corresponding to the equal arches GD (fig. 1.); and let GM, DM, be drawn so as to coincide with the plane of the excavation in such case. The breadth of the nucleus being commonly equal to that of the surrounding umbra, if the base MD of the triangle GDM conceived rectilinear be divided in L, so as  $ML : LD :: MD : DG$ ; and if through L be drawn LS parallel to DG, then will DGSL be the section of two spots having this condition, and which as to sense would, when far away from the limb, be equal in all apparent measures ;



measures; though very unequal in the third dimension HE or depth of the nucleus SL, and also in the inclination DGM of their sides to the spherical surface of the sun. Now it is manifest, from the construction of the figure, that the distances AB, AK, from the limb A, when the sides GS of the umbra disappear, must depend very much upon the last of these two circumstances; and that, according as the angle of inclination DGM is less, the respective spot will go nearer to the limb than the other before the side of the umbra GS vanishes. But those very exceptions to the general phenomena which we are at present examining are of this kind, and may, perhaps, from what has been now shewn, proceed wholly from the shallowness and the very gradual shelving of some few spots which break out in certain tracts of the sun's body over which the luminous matter lies very thinly mantled.

If, therefore, upon such principles it can be shewn, that spots, similar to the rest, may sometimes go to the limb without the one umbra contracting sensibly more than the other, the objection we are at present considering will be entirely removed, and it will be allowable to conclude, that even these few spots are excavations like all the rest, though shallower, as it would be quite unphilosophical to multiply distinctions concerning their nature, where there is found no necessity for so doing.

In order to avoid circumlocution, we may call that side of the umbra which lies nearest the limb the *nearest umbra*, and the side opposite the *farthest umbra*; and to enter more particularly into the consideration now before us, let us suppose a spot of 40'' over all, with its nucleus and umbra equally broad; then will the depth of the nucleus and the apparent breadth of the nearest umbra, when the plane of the farthest comes to coincide with the visual ray, be as expressed in the following examples,

examples, in which the apparent semi-diameter of the sun is supposed 16', and his parallax 8''.5,

	Farthest umbra supposed to vanish when distant from the limb.	Depth of nucleus in English miles and in seconds.	Apparent breadth of nearest umbra.
I.	1' 0" -	4.54 - 2118	8.58.
II.	0 30' -	3.09 - 1442	6.02.
III.	0 15' -	2.09 - 975	4.13.
IV.	0 8' -	1.44 - 672	2.87

Now, because in every aspect of a spot, the real breadth of either the farthest or nearest umbra must be to the projected or apparent breadth, as radius to the sine of the angle which this respective plane makes with the visual ray, it follows, that at any time before the spot comes so near the limb as is expressed in the above examples, the apparent breadth of the nearest and farthest umbra cannot differ so much as by the quantity there set down for the apparent breadth of the nearest, when the other is supposed to vanish. Regarding, therefore, the farthest and nearest umbra of the spot in case IV. as two neighbouring visible objects which turn narrower by degrees as the spot goes toward the limb, we should undoubtedly judge that they contract as to sense alike, since so long as the farthest could be perceived, the other cannot appear to exceed it by a quantity that we could distinguish; and by the time the plane of the former coincides with the visual ray, the extreme nearness to the limb would prevent our forming any certain judgement of either.

From this last example, therefore, it appears manifest that a spot, answering to the description and conditions there mentioned, or one a little more shallow, would approach the limb, and

and finally go off the disk, without that peculiar change of the umbra on one side, which is so obvious on common occasions, notwithstanding it were an excavation, whose nucleus or bottom is so many miles below the level of the surface.

In the four cases above stated, the distance of the remotest part of the nucleus from the sun's limb when the visual ray coming from it is just interrupted by the lip of the excavation, or, in other words, the distance of the nucleus from the limb when it is totally hid was also computed. These distances are as follow :

Case 1. - 16.93

2. - 8.90

Case 3. - 4.70

4. - 2.70

and it is remarkable from the two last, how very near the limb a shallow spot of not more than 40'' in diameter may come before the nucleus wholly disappears.

Computations of this kind are very easily made, by supposing on (fig. 2.) GDLS a section of a spot so near to the limb A, that the visual ray VB coincides with GS the plane of the excavation. Let the straight line DF coincide with the other side, and draw the radii CA, CQ, so as to be at right angles to GB, DF, and draw the radius CH through the point M.

Now, because the versed sine AB, the apparent distance from the limb, when the side of the umbra GS vanishes is given, the arch GA of the sun's circumference is given, and from the known breadth of the spot, the arch GD and its half GH are each of them given, and consequently the arches HA, DA, and QA, are all given. From these data the angles and sides of the triangle GMD, supposed rectilinear, may be deduced, also HM the distance of the point of intersection M from the surface. When these particulars have been determined for any assumed distance BA, and assumed extent of the spot

spot GD, the depth of the nucleus HE, may be found by the following analogies.

By similar triangles  $MD : DG :: ML : LS$ . But LS being the breadth of the nucleus is, by hypothesis, equal to LD, the breadth of the umbra; therefore  $MD : DG :: ML : LD$ , or as  $ME : EH$  on account of the parallels SL and GD. By composition,  $MD + DG : DG :: MH : HE$ . Hence HE, the only unknown quantity of these proportionals, is found, and is the depth of the nucleus sought. If DP, the apparent breadth of DL, the side of the umbra next the limb be required, corresponding to the present aspect of the spot, this may be derived from the data by the resolution of the triangle DLP, whose hypotenuse DL and angle DLP, are known. Again, if the nearest apparent distance of the nucleus from the limb, when it wholly disappears, be sought, it will come out equal to the versed sine of the arch GA, when diminished by half the arch HA, which last is by construction equal to half of either of the acute angles of the triangle DGM. In order to see the reason of this, it must be considered, that the segments ML, LD, of the base MD, are in the same ratio with the sides DG, GM; on which account a straight line drawn from G to L, the last part of the nucleus which can be seen, must bisect the angle DGM. Therefore, before the visual ray or line ZGL can fall perpendicularly upon the radius CA, this must be drawn distant from the point A towards D, by an arch corresponding to half the angle DGM, that is, by an arch equal to half of HA.

Perhaps it may be urged, that very shallow spots ought always to be known from the rest, and to discover themselves, by a surrounding umbra very narrow compared to the extent of the nucleus; but we know far too little of the qualities of the luminous matter, and of the proximate causes of the spots,

to say any thing at all upon a point of this kind. The breadth of the umbra is, as assumed in the computations, commonly about equal to that of the nucleus, though sometimes it varies more or less; but how far these relative dimensions indicate depth or shallowness must be expounded only by observation, and not by any vague or imperfect notions of the nature and constitution of the sun.

The mention of a pit or hollow or excavation several thousands of miles deep, reaching to that extent down through a luminous matter to darker regions, is ready to strike the imagination in a manner unfavourable to a just conception of the nature of the solar spots as now described. Upon first thoughts it may look strange, how the sides and bottom of such vast abysses can remain so very long in sight, whilst by the sun's rotation they are made to present themselves more and more obliquely to our view. But when it is considered, how extremely inconsiderable their greatest depth is, compared to the diameter of the sun, and how very wide and shelving they are, all difficulties of this sort will be entirely removed.

Unless, however, we duly attend to these proportions, our notions upon the subject must be very erroneous; and it seems the more necessary to offer this caution, as this very thing is inaccurately represented in fig. 9. belonging to the *Memoire* under review, and in a way that may lead to mistakes. Instead of exhibiting a spot as depressed below the surface of the sun one hundredth part of his semi-diameter, the section of it is there determined by two lines drawn from the circumference, and meeting in a point at the prodigious distance of one-fifth of the semidiameter below. This author's known clear and comprehensive ideas of every thing relating to the sphere have doubtless led him to think, that any particular attention to exactness was unnecessary in this representation; but

but as my design, on the present occasion, is to write and to explain matters in a popular way, rather than to astronomers, it will be proper to assist the conceptions of those who are but little versed in mathematical principles by such diagrams as will shew things in their just proportions. Any reader, therefore, who pleases, by turning to fig. 3. may see how very small a portion of the sun's body is made up of the luminous matter when supposed every where 3967 English miles deep. Fig. 3. A is a section of a spot of 50'' diameter situated in the deepest part of this resplendent substance.

For my own amusement I have pursued this subject further in the way of ocular proof, by a model of the sun and of the spots upon his body according to their proper dimensions. This I put into a convenient wooden frame, and viewed it afar off when set upon a stand, whilst the globe was turned slowly round, and subtended an angle at the telescope equal to the apparent diameter of the sun. By an object-glass micrometer I then took the distances from the limb when the farthest umbrae of different spots vanished, as also the distances of the nuclei just when disappearing. The apparent subtense of the umbra next the limb was also measured in this way, together with the visible extension of some great spots within the disk, when the extreme limits of the nearest umbra coincided with the limb. In all these experiments the effect was very striking, and the phenomena remarkably consonant to calculation, and to what I have often seen upon the real sun in the heavens.

The globe I got made consists within of two strong hollow hemispheres, formed by pasting slips of paper upon a well-turned ball of wood, and afterwards fastened together upon an iron axis in the way commonly practised. Over this were repeatedly laid coats of Spanish white and glue, applied when in a thick paste, till at length this outward shell

became of a considerable thickness. To give the whole a true form, the two projecting poles were locked up in two grooves when coinciding with the diameter of an iron semi-circle, whose inner edge was so fastened as to cut away the redundant parts of the last coat of the chalk nearly dry, whilst the globe was continuedly and slowly forced round. By thus repeatedly paring off the protuberances, and supplying new paste when deficient, and forcing the globe round against the cutting edge as before, it at length became quite smooth and spherical. After this, when slowly dried, it turned very white, and then the spots or excavations were made in its surface by boring instruments of steel as in fig. 4. constructed in all their cutting edges from a scale of parts of the globe's diameter. This done, I penciled the bottom of the hollows all over black with China ink, and distinguished the shelving sides from the full whiteness of the outward surface by a shade of the pencil which was darkest towards the external border. I hope the indulgent reader will excuse me for having been so particular in regard to this artificial sun, as possibly what I have mentioned may facilitate a like construction, should any person deem it worth while to entertain himself with such experiments.

But to proceed; what has now been insisted on at so much length concerning the shallowness and the more gradual shelving of some few spots, will also apply to another objection, which M. DE LA LANDE views in a strong light.

Here we find quoted the great spot in 1719, seen by M. CASSINI; and, for the second time, that of June 3, 1703, seen by M. DE LA HIRE; both which, on their arrival at the limb, are said to have made an indentation or dark notch in the disk; and this phenomenon is mentioned as absolutely incompatible with spots being below the surface.

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It is most true, that if we look for any thing like this, when the plane which coincides with the external boundary of the spot passes through the eye (the way that M. DE LA LANDE considers the matter, vide his fig. 9th) it must be very large indeed before the disk could be perceived deficient by any dark segment. But may not a spot, even no larger than M. CASSINI's, considered as an excavation, make, in a manner very different from this, something like a notch; for, by the way, this phenomenon is not in the Mem. Acad. nor any where else, that I know of, described with any sort of precision:

M. CASSINI's great spot, by which we understood the nucleus, was one of 30''; and supposing the umbra equally broad, its diameter over all must have been 1' 30''. It would, therefore, occupy an extent upon the sun's surface of 5° 22' fully. Now, suppose a circular space of that size upon the sun, distinguished from the surrounding lustre by such a failure of light as is peculiar to some spots, and suppose that it just touches the limb, it would still subtend an angle of more than 4''. This being the case, might not a dusky shade, more or less remarkable, according to the darkness of the umbra, commencing at the limb, and reaching inwards upon the disk, or, in other words, a notch be perceived? Had M. CASSINI's spot been a very shallow excavation, it appears by case fourth, formerly stated, that when viewed in this aspect, some small part of the nucleus might have been yet visible, and might have contributed along with the shade of the farthest umbra, and the still deeper and broader shade of the two ends of the umbra, to mark out the indentation.

Should it be said, that these notches are always distinct jet black impressions on the disk, of an obvious breadth, and originating entirely from the opaque nucleus conceived as something



thing prominent above the general surface, this can be shewn inconsistent with some circumstances we find accidentally mentioned in the case of M. DE LA HIRE's spot: for of this great one, it is said, that when only 8'' distant from the limb, the nucleus was seen as a very narrow line. This was on June 3, 1703, at six o'clock in the morning. Now, forasmuch as, at that time, its alledged elevation must have been to its apparent subtense, very nearly, as radius to cosine of that arch of the sun's circumference whose versed sine was the 8'' of distance from limb, it is impossible that its breadth could have increased sensibly in its further progress towards the limb; and how any obvious black notch could be produced by the elevation contended for in this case, is not conceivable.

I do not imagine, therefore, that the phenomena of notches in the disk, so inconsiderable and dubious as these seem to be, are by any means a proof of projecting nuclei, or that they are not reconcileable to spots being depressions in the sun. A large shallow excavation, with the sloping sides or umbra darker than common, may, as has been shewn, be more or less perceptible at the limb: and what, perhaps, is a further confirmation of this, and seems to evince that a concurrence of such circumstances is necessary, is, that sometimes even large spots make no indentation. M. CASSINI, in *Mem. Acad.* tom. X. p. 581. describes the great spot of 1676, which he saw at its entrance with a telescope of 35 feet, as an obscure line parallel to the limb, but no where mentions that it made a notch in it.

Though we now and then see the surrounding umbra darker than at other times, yet when spots are deep, and the umbra but little dusky, it is indeed impossible that we should see anything of them, even though large, very near the limb: nor here even the nucleus which lies buried cannot in the last

contribute to the effect, as it may do a little before its state of evanescence when spots are very shallow: accordingly cases of this kind are perfectly agreeable to experience. This fact seems to have met SCHEINER as a stumbling-block, when he was intent upon bringing forward every possible argument for the spots being something which project beyond the surface. "Non raro contingit," says he, *Rosa Urina*, p. 511. "ut magnæ folis maculæ antequam ad horizontem accedant, repente ita luce obruantur, ut omnino videri nequeant."

In reasoning concerning the nature of the spots, and particularly about their third dimension, the only arguments which are admissible, and which carry with them a perfect conviction, are those grounded upon the principles of optical projection. If, for example, by far the greater number of them be excavations, some thousands of miles deep, certain changes of the umbra would be observable when near the limb, as has been shewn at so much length. Were they very shallow, or quite superficial, both sides of the umbra would as to sense contract alike in their progress toward the limb: for if in case 4th, above stated, the spot had been supposed superficial, the apparent breadth of the side of the umbra next the center of the disk, would have then been only  $1''.62$ , and that of the side opposite  $1''.27$ . Now, the whole of either of these quantities, and much more their difference, would be quite insensible. Again, if the nucleus extended much above the common level whilst the surrounding umbra was superficial, we should behold manifest indications of this by such an opaque body when seen very obliquely being projected across the farthest side of the umbra, and by hiding the whole or part of it before the time it would otherways disappear. According to this or that condition of the spot, such things must infallibly obtain  
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by the known laws of vision; and hence arguments resting upon such principles may be denominated *optical* ones. On the other hand, when spots are contemplated near the middle of the disk, a great variety of changes are observed in them, which depend not upon position, but upon certain physical causes producing real alterations in their form and dimensions. It is plain, that arguments derived from the consideration of such changes, and which, on that account, may be called *physical arguments*, can assist us but little in investigating their third dimensions; and, from the nature of the thing, must be liable to great uncertainty. The author of the *Memoire*, in p. 511. &c. takes new ground, and proceeds with a number of objections, depending upon that sort of reasoning which we have last defined. I must take notice, that a certain distinction has been here overlooked, which in my paper I endeavoured to point out. Presuming upon our great ignorance of many things which doubtless affect deeply the constitution of that wonderful body the sun, I offered, in part II. an account of the production changes and decay of the spots, considered as excavations, in the most loose and problematical manner, stating every thing on this head in the form of queries. This account, crude and imperfect as it is, appeared to me much less incumbered with difficulties than any other, and of this some striking examples are there set before the reader. But I have expressly owned, that many circumstances still remained unexplained; and upon the whole marked out the theory, if such it may be called, as very imperfect. Nature unquestionably abounds with numberless unthought-of energies, and modes of working most curiously and most wisely adapted to all situations in the material world: and in regard to that system of economy which is established in the sun, producing there

there such a strange fluctuation of appearances, human reason, even when aspiring by the most enlarged analogy, must recoil under a consciousness of the unfathomable resources of nature, and of its own dark and limited sphere. “*Demiraberis qui-  
“ dem sine dubio*” (says HEVELIUS, speaking of the Sun, *Cometographia*, p. 412.) “*quod tam brevi tempore, spatio ali-  
“ quot dierum, quin horarum, adeo miris et horribilibus sub-  
“ jiciantur mutationibus, ac vicissitudinibus!*”

Hence I would remark, that whatever inconsistencies are imagined in the account I have delivered in part II. though such may be justly chargeable upon certain principles there assumed, yet they ought not to be stated as presumptions against the spots being really excavations or depressions in the luminous matter of the sun. This opinion must rest entirely upon the evidence held forth in the first part of the paper, whatever be the fate of the account laid down in the second. It does not enter there as an hypothesis, but as a matter of fact, previously established by *optical arguments*; and from optical arguments alone can there arise even any just presumptions against it. The lameness of the views given in part II. may probably proceed, as we have said, from our very imperfect knowledge of the vast range of physical causes which obtain in the universe. But whatever be their defects, no doubts ought to arise, upon such grounds, of the spots being themselves what direct observation declares them, namely, excavations in the sun. Whether their first production and subsequent numberless changes depend upon the eruption of elastic vapour from below, or upon eddies or whirl-pools commencing at the surface, or upon the dissolving of the luminous matter in the solar atmosphere, as clouds are melted and again given out by our air; or, if the reader pleases, upon the annihilation

and reproduction of parts of this resplendent covering; is left for theory to guess at. Though, however, many difficulties should occur in an attempt of this kind, it would certainly be unreasonable on that account to call in question the third dimension of the spots, as previously determined by arguments which are liable to no fallacy, and which are unconnected with every kind of theoretical reasoning.

Now, in the Memoire before me, this sort of distinction has escaped the notice of the author. His optical arguments, indeed, as they regard the first part of my essay, put on a just and proper claim to be heard, and have now, as we conceive, been fully answered. But superadded to these are many others, which, though they relate very properly to the view I have given in part II. and to that alone, yet finally are summed up along with the rest, as not only militating strongly against that particular view, but in order to disprove that the spots are excavations in the luminous matter.

I here think it but justice to that honourable Body of Gentlemen who, in the year 1774, composed the Council of the Royal Society, or the Committee of Papers, to mention, that the publication of the second part of mine was more owing to their having consented to my request, than to their own sentiments in regard to the fitness of so doing. But as I had bestowed some pains upon drawing up these views, and as care had also been taken to distinguish between fact and any thing like to theory, and as the latter was propounded only in the form of queries, there appeared to me no harm in letting that second part go forth also; especially as I flattered myself, that thereby a greater curiosity would have been excited, and the subject of course sooner inquired into by observation.

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As I conceive it, however, of some importance to have the distinction above treated of perfectly understood in future, I now purposely avoid entering upon any theoretical ground whatever. My wish therefore is, that the author of the *Memoire* may acquit me of every thing not perfectly respectful, though I do not follow him through that train of objection founded upon vague and incompetent physical arguments, which is to be met with in p. 511. &c. By further considering the particulars hinted at in p. 21. and 29. of my paper, several difficulties, perhaps, may be removed; but we forbear any illustration of this kind, chiefly to evince how little we concern ourselves whether the views delivered in part II. can stand of themselves or not. Those who do not like the principles there assumed, or the conclusions drawn from them; in short, those who will call part II. a theory, and who think it a bad one, may, if they please, mend it, or contrive a new and a better one of their own. But so long as they cannot, by irrefragable optical arguments, set aside the induction laid down in part I. we must demand of them, so to fabricate their theories as to account for the various circumstances of the spots, considered as things which possess three dimensions, *viz.* length, breadth, and depth, or, in other words, as excavations in the luminous matter of the sun.

This fact is the only one I am solicitous to maintain or to contend for; and for a very good reason, because I consider it as actually demonstrated by competent observations. As such, to indulge for a moment in a figure, it would be a pity not to rescue it from being drawn into the eddy of some treacherous theory, the nature of all which is to sweep into their vortex and finally to precipitate to the bottom every thing which obstructs their impetuous career.

Sir ISAAC NEWTON, perceiving too well this proneness to system, has laid down his fourth rule of philosophizing, that arguments of induction may not be evaded by hypothesis. It will become us, therefore, in all things, and in the present subject in particular, to have respect to so excellent a precept. In speaking hereafter of the solar spots, let us separate what things claim to be heard as matters of fact from what rest upon the sandy foundations of mere theory, and no longer confound them together.

Since upon this topic, I humbly beg the indulgence of the reader whilst I advert to a certain abuse of terms, which is but too prevalent in books of philosophy, both in our own country and upon the continent. What I have to say relates to this word *hypothesis*. “*Quicquid enim non deducitur ex phænomenis hypothesis vocanda est,*” are the words of Sir ISAAC NEWTON in his general scholium. And yet real discoveries, founded upon the best induction, are sometimes mentioned by the appellation of such and such a one's hypothesis. I have often thought, that this impropriety of language owes its continuance to the force of custom, and that it is one of those badges we still retain of that disgraceful state philosophy lay under before the æra of experiment and observation, when almost every thing was hypothesis and theory both in name and in reality.

Most kind of hypothesis regards true philosophy with so unfriendly an aspect, that we should be careful at least not to contaminate matter of fact and certain truth with so inauspicious a denomination. I would also remark, that none which do not carry with them great marks of probability should be brought into view, even in the way of hints or queries, for suggesting further experiments and observations; and that far

less ought systems, built upon notions evidently incongruous, to have a place in any modern book of philosophy. This has a tendency still to favour that devious path, that *false taste*, which it concerns philosophy so much to guard against and to discourage.

It remains now only to make a few strictures upon M. DE LA LANDE's theory of the solar spots, humbly submitting them to the consideration of the reader. The import of it is, "that the spots as phenomena arise from dark bodies like rocks, which by an alternate *flux* and *reflux* of the liquid igneous matter of the sun, sometimes raise their heads above the general surface. That part of the opaque rock, which at any time thus stands above, gives the appearance of the nucleus, whilst those parts, which in each lie only a little under the igneous matter, appear to us as the surrounding umbra."

In the first place it may be remarked, "that the whole proceeds upon *mere supposition*." This, indeed, the author himself very readily acknowledges. Though, therefore, it could not be disputed by arguments derived from observation, yet conjecture of any kind, if equally plausible, might fitly be employed to set aside its credit. Choosing, however, to avoid a tedious discussion of this kind, or to try it upon the phenomena which are enumerated in p. 511. &c. by entering into arbitrary and disputable principles, we shall confine ourselves to such particulars as appertain to the more obvious character of the spots, and which also seem to be irreconcilable with the theory; and first of all, in regard to the distinguishing features of the umbra.

M. CASSINI, Mem. Acad. tom. X. p. 582. plate 7. and M. DE LA HIRE, Mem. Acad. 1703, p. 16. and I may add all other observers, and all good representations of the spots, bear testi-



testimony to the exterior boundary of the umbra being always well defined, and to the umbra itself being less and less shady the nearer it comes to the nucleus. Now it may be asked, how this could possibly be according to M. DE LA LANDE's theory? If the umbra be occasioned by our seeing parts of the opaque rock, which lie a little under the surface of the igneous matter, should it not always be darkest next the nucleus, and from the nucleus outward should it not wax more and more bright, and at last lose itself in the general lustre of the sun's surface, and not terminate all at once at the darkest shade, as in fact it does? These few incongruities, which meet us as it were in the very threshold of the theory, are so very palpable, that of themselves they raise unsurmountable doubts. For, generally speaking, the umbra immediately contiguous to the nucleus, instead of being very dark, as it ought to be, from our seeing the immersed parts of the opaque rock through a thin stratum of the igneous matter, is on the contrary very nearly of the same splendour as the external surface.

Concerning the nucleus, or that part of the opaque rock which stands above the surface of the sun, M. DE LA LANDE produces no optical arguments in support of this third dimension or height. Neither does he say any thing particular as to the degree of elevation above the surface. But from what has been already hinted in the course of this paper, it appears, that if this were any thing sensible, it ought to be discovered by phenomena very opposite to those which we have found to be so general.

Again, a flux and reflux of the igneous matter so considerable as sometimes to produce a great number of spots all over the middle zone, might affect the apparent diameter of the sun, making that which passes through his equator less than  
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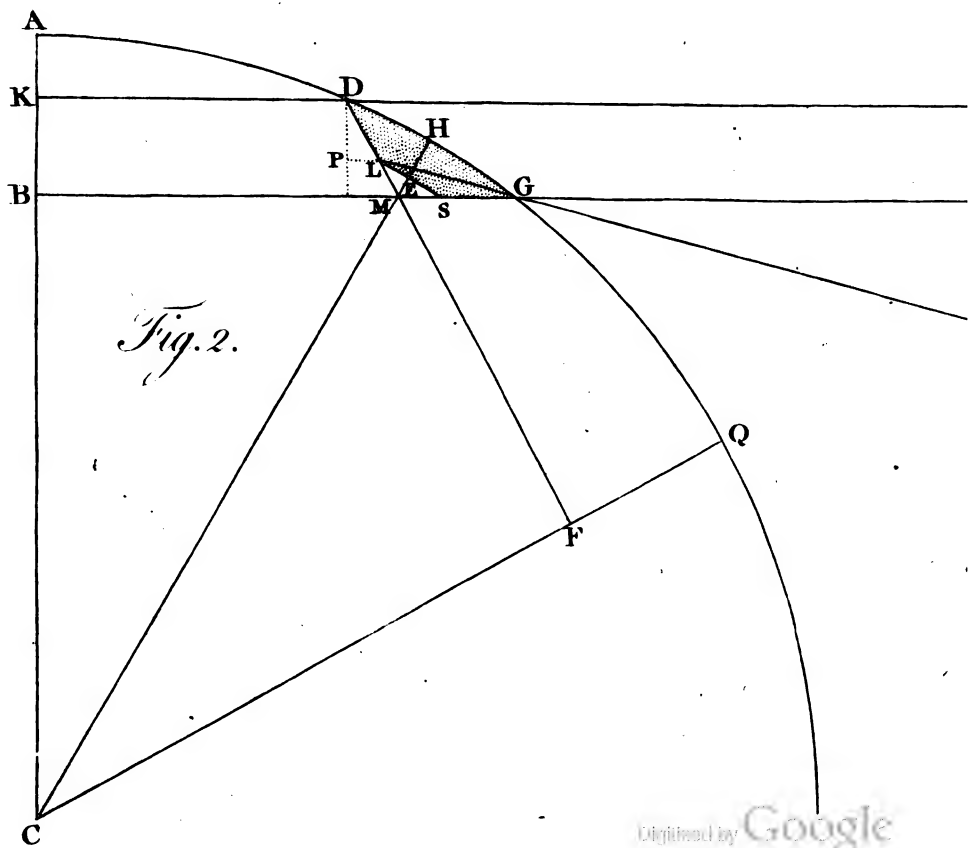
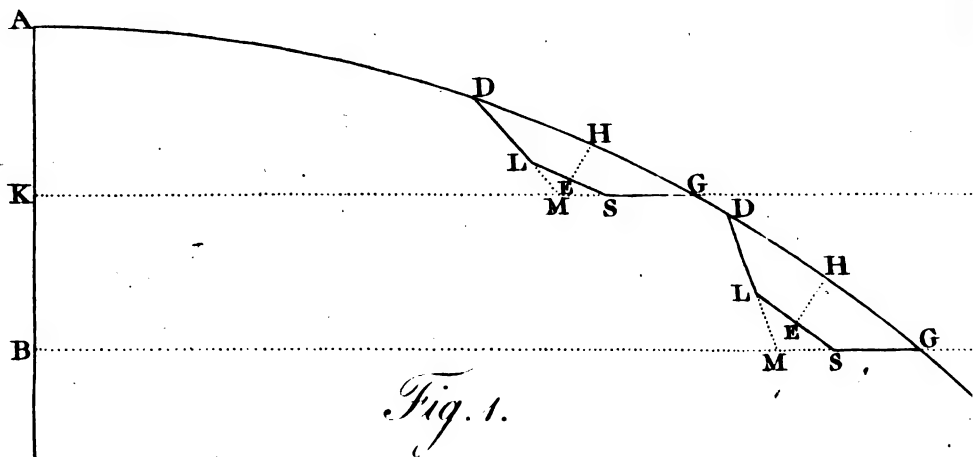
the polar one, by the retreat of the igneous matter towards those regions where no spots ever appear. But as a difference of this kind of nearly a thousandth part of the whole would be perceivable, as we learn from M. DE LA LANDE's own observations, compared with those of Mr. SHORT, in *Histoire Acad.* 1760, p. 123. it would seem, that the theory had also this difficulty to combat. Further, when among spots very near one another, some are observed to be increasing, whilst others are diminishing, how is it possible that this can be the effect of such a supposed flux and reflux? This last inconsistency is mentioned by the author himself, who endeavours to avoid it, by making a new demand upon the general fund of hypothesis, deriving from thence such qualities of the igneous matter as the case seems to require; and such must be the method of proceeding in all systems merely theoretical.

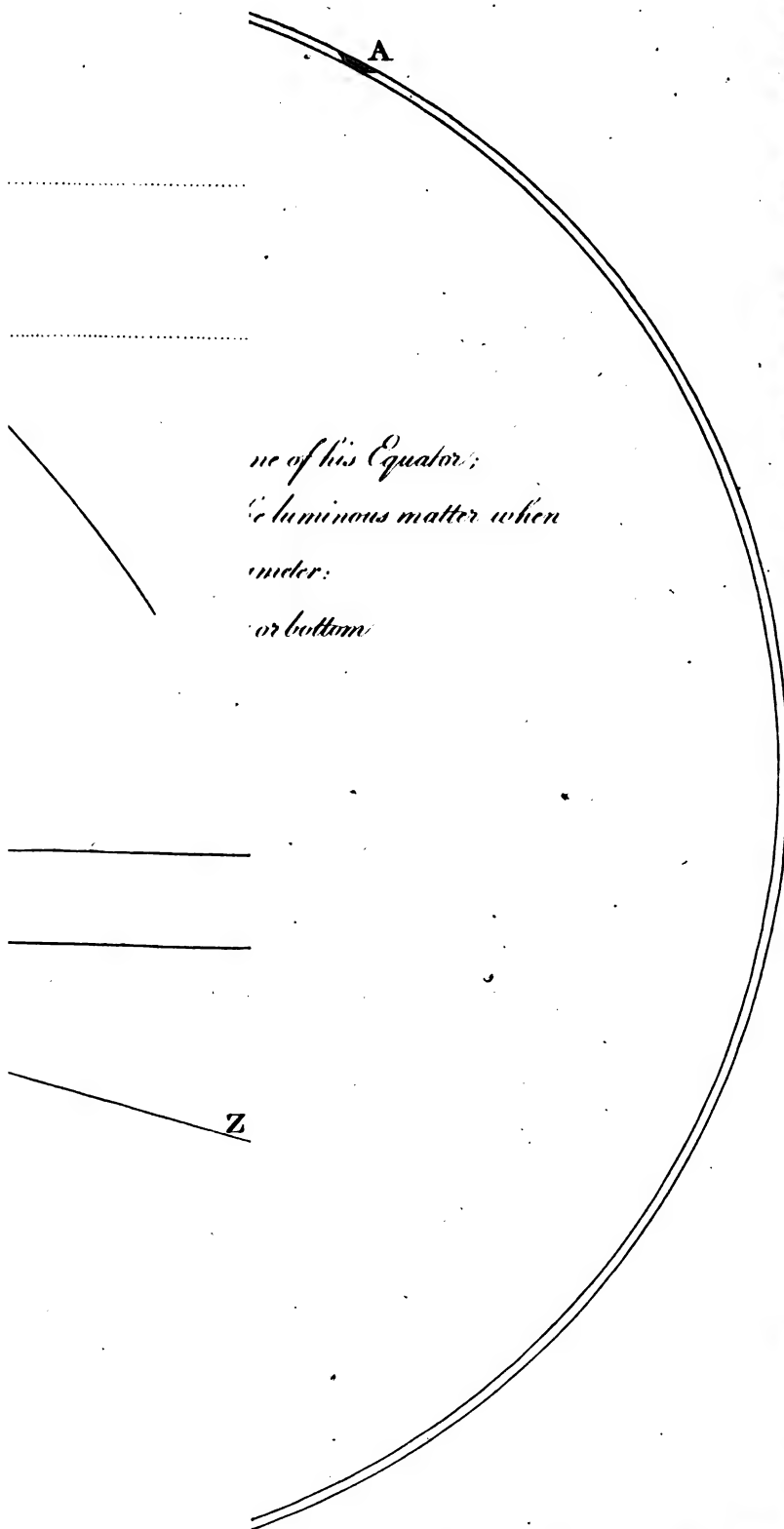
But it is unnecessary to pursue at more length illusive speculations of this kind, especially as we lie under a conviction, founded upon fact, of the theory being utterly erroneous. It hardly differs in any respect from that proposed by M. DE LA HIRE, and a little amended by the writer of the *Histoire de L'Acad.* for 1707, p. 111. This near agreement, indeed, is taken notice of by M. DE LA LANDE himself, in his excellent astronomy.

The writer of the *Histoire de L'Acad.* for 1719, p. 76. after reviewing the merits of this theory, and comparing it with several phenomena of the spots which had been observed for the four preceeding years, pronounces it unsatisfactory, and concludes his remarks with the following expression: “ Il sera plus naturel de croire qu'il se fasse dans le soleil des generations nouvelles, dependantes de quelque cause plus ou moins forte selon les *circonstances inconnues* ou elle se trouvera.”

Views,

Views, much of the same kind, were even entertained by some so long ago as the days of SCHEINER, as we find mentioned by that indefatigable author in his *Rosa Ursina*, p. 746. “Non equidem me latebat,” says he, “non deesse, qui putant  
“maculas solares esse quasdam in sole prominentias et *veluti*  
“*montes*; sed cum hæc ex anticipata mentis affectione,  
“phænomenique solaris ignoratione procedant neglecti.” And further on he adds: “Astronomi sinceri est phænomenon sequi  
“non antevertere: veritatem ex objecto accurate observato,  
“non objectum invitum ad arbitraria figmenta trahere.”





**XI.** *An Account of the Earthquakes which happened in Italy, from February to May 1783. By Sir William Hamilton, Knight of the Bath, F. R. S. in a Letter to Sir Joseph Banks, Bart. P. R. S.*

Read July 3, 1783.

Naples, May 23, 1783.

**I** AM happy now to have it in my power to give you, and my brethren of the Royal Society, some little idea of the infinite damage done, and of the various phenomena exhibited, by the earthquakes (which began the 5th of February last, and continue to be sensibly, though less violently, felt to this day) in the two Calabrias, at Messina, and in the parts of Sicily nearest to the continent. From the most authentic reports, and accounts received at the offices of his Sicilian Majesty's secretary of state, we gathered in general, that the part of Calabria, which has been most affected by this heavy calamity, is that which is comprehended between the 38th and 39th degree, that the greatest force of the earthquakes seemed to have exerted itself from the foot of those mountains of the Apennines called the Monte Deio, Monte Sacro, and Monte Caulone, extending westward to the Tyrrene sea; that the towns, villages, and farm-houses, nearest these mountains, situated either on hills or in the plain, were totally ruined by the first shock of the 5th of February about noon; and that the

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greatest mortality was there; that in proportion as the towns and villages were at a greater distance from this center, the damage they received was less considerable; but that even those more distant towns had been greatly damaged by the subsequent shocks of the earthquake, and especially by those of the 7th, the 26th, and 28th of February, and that of the 1st of March; that from the first shock, the 5th of February, the earth continued to be in a continual tremor, more or less; and that the shocks were more sensibly felt at times in some parts of the afflicted provinces than in others; that the motion of the earth had been various, and, according to the Italian denomination, *vorticoso*, *orizontale*, and *oscillatorio*, either whirling like a vortex horizontal, or by pulsations, or beatings from the bottom upwards; that this variety of motion had increased the apprehensions of the unfortunate inhabitants of those parts, who expected every moment that the earth would open under their feet, and swallow them up; that the rains had been continual and violent, often accompanied with lightning and irregular and furious gusts of wind; that from all these causes the face of the earth of that part of Calabria (comprehended as abovementioned between the 38th and 39th degrees) was entirely altered, particularly on the westward side of the mountains above named; that many openings and cracks had been made in those parts; that some hills had been lowered, and others quite levelled; that in the plains, deep chasms had been made, by which many roads were rendered impassable; that huge mountains had been split asunder, and parts of them driven to a considerable distance; that deep vallies had been filled up by the mountains (which formed those vallies) having been detached by the violence of the earthquakes, and joined together; that the course of some rivers had been altered;

that many springs of water had appeared in places that were perfectly dry before ; and that in other parts, springs that had been constant had totally disappeared ; that near Laureana in Calabria Ultra, a singular phenomenon had been produced, that the surface of two whole tenements, with large olive and mulberry-trees therein, situated in a valley perfectly level, had been detached by the earthquake, and transplanted, the trees still remaining in their places, to the distance of about a mile from their first situations ; and that from the spot on which they formerly stood hot water had sprung up to a considerable height, mixed with sand of a ferruginous nature ; that near this place also some countrymen and shepherds had been swallowed up with their teams of oxen and their flocks of goats and sheep ; in short, that beginning from the city of Amantea, situated on the coast of the Tyrrene sea in Calabria Citra, and going along the westward coast to Cape Spartivento in Calabria Ultra, and then up the eastern coast as far as the Cape d'Alice (a part of Calabria Citra on the Ionian sea), there is not a town or village, either on the coast or land, but what is either totally destroyed, or has suffered more or less, amounting in all to near four hundred, what are called here *Paeses* ; a village containing less than an hundred inhabitants is not counted as a *Paese*.

The greatest mortality fell upon those towns and countries situated in the plain on the western side of the mountains Dejo, Sacro, and Caulone. At Casal Nuovo, the Princess Gerace, and upwards of 4000 of the inhabitants, lost their lives ; at Bagnara, the number of dead amounts to 3017 ; Radicina and Palmi count their loss at about 3000 each ; Terranuova about 1400 ; Seminari still more. The sum total of the mortality in both Calabrias and in Sicily, by the earthquakes



alone, according to the returns in the secretary of state's office at Naples, is 32,367; but I have good reason to believe that, including strangers, the number of lives lost must have been considerably greater, 40,000 at least may be allowed, and, I believe, without any exaggeration.

From the same office intelligence we likewise heard, that the inhabitants of Scilla on the first shock of the earthquake, the 5th of February, had escaped from their houses on the rock, and, following the example of their prince, taken shelter on the sea-shore; but that in the night-time the same shock, which had raised and agitated the sea so violently, and done so much damage on the point of the Faro of Messina, had acted with still greater violence there, for that the wave (which was represented to have been boiling hot, and that many people had been scalded by its rising to a great height) went furiously three miles inland, and swept off in its return 2473 of the inhabitants of Scilla, with the prince at their head, who were at that time either on the Scilla Strand, or in boats near the shore.

All accounts agreed, that of the number of shocks which have been felt since the beginning of this formidable earthquake, amounting to some hundreds, the most violent, and of the longest duration, were those of the 5th of February at 19½ (according to the Italian way of counting the hours); of the 6th of February, at 7 hours in the night; of the 27th of February, at 1½ in the morning; of the first of March, at 8½ in the night; and that of the 28th of March, at 1½ in the night. It was this last shock that affected most the upper part of Calabria Ultra, and the lower part of the Citra, an authentic description of which you will see hereafter, in a letter which I received from the Marquis Ippolito, an accurate observer residing

residing at Catanzaro in the upper Calabria. The first and the last shocks must have been tremendous indeed, and only these two were sensibly felt in this capital.

The accounts which this government has received from the province of Cosenza, are less melancholy than those from the province of Calabria Ultra. From Cape Suvero to the Cape of Cetraro on the western coast, the inland countries, as well as those on the coast, are said to have suffered more or less in proportion to their proximity to the supposed center of the earthquakes; and it has been constantly observed, that its greatest violence has been exerted, and still continues to be so, on the western side of the Appennines, precisely the celebrated *Sila* of the ancient *Brutii*, and that all those countries situated to the eastward of the *Sila* had felt the shocks of the earthquake, but without having received any damage from them. In the province of Cosenza there does not appear to be above 100 lives lost. In the last accounts from the most afflicted part of Calabria Ultra, two singular phenomena are mentioned. At about the distance of three miles from the ruined city of Oppido, there was a hill (the soil of which is a sandy clay) about 500 palms high, and 1300 in circumference at its basis. It was said, that this hill, by the shock of the 5th of February, jumped to the distance of about four miles from the spot where it stood into a plain called the *Campo di Bassano*. At the same time the hill on which the town of Oppido stood, which extended about three miles, divided in two, and as its situation was between two rivers, its ruins filled up the valley, and stopped the course of those rivers; two great lakes are already formed, and are daily increasing, which lakes, if means are not found to drain them, and give the rivers their due course, in a short time must infect the air greatly.

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From Sicily the accounts of the most serious nature were those of the destruction of the greatest part of the noble city of Messina, by the shock of the 5th of February, and of the remaining parts by the subsequent ones;—that the kay in the port had sunk considerably, and was in some places a palm and a half under water;—that the superb building, called the Palazzata, which gave the port a more magnificent appearance than any port in Europe can boast of, had been entirely ruined;—that the Lazaret had been greatly damaged; but that the citadel had suffered little;—that the mother church had fallen; in short, that Messina was no more;—that the tower at the point of the entrance of the Faro was half destroyed; and that the same hot wave, that had done such mischief at Scilla, had passed over the point of land at the Faro, and carried off about 24 people. The viceroy of Sicily likewise gave an account of some damage done by the earthquakes, but nothing considerable, at Melazzo, Patti, Terra di Santa Lucia, Castro Reale, and in the island of Lipari.

This, Sir, was the intelligence I was possessed of the end of last month; but as I am particularly curious, as you know, on the subject of volcanoes, and was persuaded in my own mind (from the present earthquakes being confined to one spot) that some great chemical operation of nature of the volcanic sort was the real cause of them; in order to clear up many points, and to come at truth, which you also well know, Sir, is exceedingly difficult, I took the sudden resolution to employ about twenty days (which was as much as I could allow, and have time to be out of Italy, in my way home, before the heats set in) in making the tour of such parts of Calabria Ultra and Sicily as had been, and were still, most affected by the earthquakes, and examining with my own eyes the phenomena above mentioned. I accordingly hired

hired for that purpose a Maltese Speronara for myself, and a Neapolitan Felucca for my servants, and left Naples the 2d of May. I was furnished, by command of his Sicilian Majesty, with ample passports, and orders to the commanding officers of the different provinces to give me every assistance and protection in the pursuit of my object. I had a pleasant voyage in my Maltese Speronara (which are excellent boats, and the boatmen very skilful) along the Coast of the Principato Citra and Calabria Citra, after having passed the gulph of Policastro. At Cedraro I found the first symptoms of the earthquake, some of the principal inhabitants of that city having quitted their houses, and living in new erected barracks, though not a house in the whole town, as I could see, had suffered. At St. Lucido I perceived that the baron's palace and the church steeple had suffered, and that most of the inhabitants were in barracks. The barracks are just such sort of buildings as the booths of our country fairs, though indeed many I have seen are more like our pig-styes. As my object was to get as fast as possible to the center of the mischief, having little time, and much to see, I contented myself with a distant view of Maida, Nicastro, and Santo Eufemia, and pushed on to the town of Pizzo in Calabria Ultra, where I landed on the evening of the 6th of May. This town, situated on the sea, and on a volcanic tuffa\*, had been greatly damaged by the earthquake of the 5th of February, but was completely ruined by that of the 28th of March. As the inhabitants of this town (amounting to about 5000) had sufficient warning, and had left their houses, and taken to barracks on the first shock the 5th of February, the mortality on the 28th of

\* This was the only token of former volcanic explosions that I met with in Calabria.

March

March was inconsiderable; but, from the barracks having been ill-constructed, and many situated in a very confined unwholesome spot, an epidemical disorder had taken place, and carried off many, and was still in fatal force whilst I was there, in spite of the wise endeavours of government to stop its progress. I fear, as the heats increase, the same misfortune will attend many parts of the unfortunate Calabria, as also the city of Messina. The inhabitants of Pizzo seemed to me to have habituated themselves already to their present inconvenient manner of living, and shops of every kind were opened in the streets of the barracks, which, except some few, are but poorly constructed. I was assured here, that the volcano of Stromboli, which is opposite, and in full view of, this town, and at the distance of about fifty miles, had smoked less, and thrown up a less quantity of inflamed matter during the earthquakes than it had done for some years past; that slight shocks continued to be felt daily; and the night I slept here, on board the *Speronara* drawn on shore, I was awakened with a smart one, which seemed to lift up the bottom of the boat, but it was not attended with any subterraneous noise. My servants, in the other boat, felt the same. The next day I ordered my boats to proceed to Reggio, and I went on horseback to Monteleone, about six miles from Pizzo, up hill, on a road of loose stones and clay, scarcely passable in this season, but through the most beautiful and fertile country I ever beheld: a perfect garden of olive-trees, mulberry-trees, fruit-trees, and vines; and under these trees the richest crops of corn or lupins, beans or other vegetables, which seemed to thrive perfectly, though under a thick shade. This is the stile of the whole plain of Monteleone, except that here and there are vast woods of oak and olive-trees mixed, and the olive-

trees

trees of such a size as I could never have conceived, being half as big as the oaks themselves, which are fine timber-trees, and more than treble the size of the olive-trees of the Campagna Felice. The olive woods, in some parts of the plain, are regularly planted in lines, and in others grow irregularly. Though the object of my present journey was merely to take a hasty view of the spots which had suffered so much by the calamity, my attention was continually called away, and I was lost in the admiration of the fertility and beauty of this rich province, exceeding by many degrees (as to the first point) every country I have yet seen. Besides the two rich products of silk and oil, in which this province surpasses every other, perhaps, in the whole world, it abounds with corn, wine, cotton, liquorice, fruit, and vegetables of every kind; and if its population and industry kept pace with its fertility, the revenue of Calabria Ultra might surely be more than doubled in a short time. I saw whole groves of mulberry-trees, the owners of which told me, did not let for more than five shillings an acre, when every acre would be worth at least five pounds, had they hands to gather the leaves and attend the silk-worms. The town of Monteleone, anciently Vibo Valentia, is beautifully situated on a hill, overlooking the sea and the rich plains above mentioned, bounded by the Apennines, and crowned by Aspromonte, the highest of them all, interspersed with towns and villages, which, alas! are no more than heaps of ruins. The town of Monteleone suffered little by the first shocks of the earthquake; but was greatly damaged by that of the 28th of March (though only twelve lives were lost), and all the inhabitants are reduced to live in barracks, many of which are well constructed with either planks or reeds, covered with plaister on the outside. As

this country has ever been subject to earthquakes, the barons had usually a barrack near their palace, to retire to on the least alarm of an earthquake. I inhabited here a magnificent one, consisting of many rooms well furnished, which was built by the present Duke of Monteleone's grand-father. I owe the safety and the expedition of the very interesting journey which I have taken through this province to this duke's goodness, as he was pleased at Naples to furnish me with a letter to his agent; in consequence of which, I was not only most hospitably and elegantly treated in his barrack, and supplied with excellent sure-footed horses for myself and servant, but also with two of his horse-guards, well acquainted with the cross roads of the country, without which it would have been impossible, with any degree of safety, to have visited every curious spot between Monteleone and Reggio, as I did, in four days. No one, that has not had the experience, can conceive the horrid state of the roads in Calabria, even in this season, nor the superior excellence of the horses of the country. All agreed here that every shock of the earthquake seemed to come with a rumbling noise from the westward, beginning usually with the horizontal motion, and ending with the vorticoſe, which is the motion that has ruined most of the buildings in this province. The same observation I found to be a general one throughout this province. I found it a general observation also, that before a shock of an earthquake, the clouds seemed to be fixed and motionless; and that immediately after a heavy shower of rain, a shock quickly followed. I spoke with many here and elsewhere, who were thrown down by the violence of some of the shocks; and several peasants in the country told me, that the motion of the earth was so violent, that the heads

heads of the largest trees almost touched the ground from side to side; that during a shock, oxen and horses extended their legs wide asunder not to be thrown down, and that they gave evident signs of being sensible of the approach of each shock. I myself observed, that in the parts that have suffered most by the earthquakes, the braying of an ass, the neighing of a horse, or the cackling of a goose, always drove people out of their barracks, and was the occasion of many pater-nosters and ave-marias being repeated in expectation of a shock. From Monteleone I descended into the plain, having passed through many towns and villages which had been more or less ruined according to their vicinity to the plain. The town of Mileto, situated in a bottom, I saw was totally destroyed, and not a house standing. At some distance I saw Soriano and the noble Dominican convent a heap of ruins; but as my object was not to visit ruins, but the greater phenomena produced by the earthquakes, I went on to Rosarno. I must, however, first mention the most remarkable instance I met with of animals being able to live long without food, of which there have been many examples during these present earthquakes. At Soriano two fattened hogs, that had remained buried under a heap of ruins, were taken out alive the forty-second day; they were lean and weak, but soon recovered. One of his Sicilian majesty's engineers, who was present at the taking them out, gave me this information. It was evident to me, in this day's journey, that all habitations situated on high grounds, the soil of which is a gritty sand stone, somewhat like a granite, but without the consistence, had suffered less than those situated in the plain, which are universally levelled to the ground. The soil of the plain is a sandy clay, white, red, or brown; but the

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the white prevails most, and is full of marine shells, particularly scollop shells. This valley of clay is intersected in many parts by rivers and torrents coming from the mountains, which have produced wide and deep ravines all over the country. Soon after we had passed through the ruined town of St. Pietro, we had a distant view of Sicily, and the summit of Mount Etna, which smoked considerably. Just before we arrived at Rosarno, near a ford of the river Mamella we passed over a swampy plain, in many parts of which I was shewn small hollows in the earth, of the shape of an inverted cone: they were covered with sand, as was the soil near them. I was told that, during the earthquake of the 5th of February, from each of these spots a fountain of water mixed with sand had been driven up to a considerable height. I spoke to a peasant here, who was present, and was covered with the water and sand; but assured me, that it was not hot, as had been represented. Before this appearance, he said, the river was dry; but soon after returned and overflowed its banks. I afterwards found, that the same phenomenon had been constant with respect to all the other rivers in the plain during the formidable shock of the 5th of February. I think this phenomenon is easily explained, by supposing the first impulse of the earthquake to have come from the bottom upwards, which all the inhabitants of the plain attest to be fact; the surface of the plain suddenly rising, the rivers, which are not deep, would naturally disappear, and the plain, returning with violence to its former level, the rivers must naturally have returned, and overflowed, at the same time that the sudden depression of the boggy grounds would as naturally force out the water that lay hid under their surface. I observed in the other parts where this sort of phenomenon had been exhibited, that

the ground was always low and rushy. Between this place and Rosarno we passed the river Messano or Metauro (which is near the town above mentioned) on a strong timber bridge, 700 palm long, which had been lately built by the Duke of Monteleone. From the cracks made on the banks and in the bed of the river by the earthquake, it was quite separated in one part, and the level on which the piers were placed having been variously altered, the bridge has taken an undulated form, and the rail on each side is curiously scolloped; but the parts that were separated having been joined again, it is now passable. The duke's bridge-man told me also, that at the moment of the earthquake, this great river was perfectly dry for some seconds, and then returned with violence, and overflowed; and that the bridge undulated in a most extraordinary manner. When I mention the earthquake in the plain, it must be always understood the first shock of the 5th of February, which was by far the most terrible, and was the one that did the whole mischief in the plain, without having given any previous notice. The town of Rosarno, with the Duke of Monteleone's palace there, was entirely ruined; but the walls remained about six feet high, and are now fitting up as barracks. The mortality here did not much exceed 200 out of near 3000. It had been remarked at Rosarno, and the same remark has been constantly repeated to me in every ruined town that I have visited, that the male dead were generally found under the ruins in the attitude of struggling against the danger; but that the female attitude was usually with hands clasped over their heads, as giving themselves up to despair, unless they had children near them; in which case they were always found clasping the children in their arms, or in some attitude which indicated their anxious care to protect them; a strong instance  
of

of the maternal tenderness of the sex! The only building that remained unhurt at Rosarno was a strong built town gaol, in which were three notorious villains, who would probably have lost their lives had they been at liberty. After having dined in a barrack, the owner of which had lost five of his family by the earthquake, I proceeded to Laureana, often crossing the wide extended bed of the river Metauro. The environs of Laureana, which stands on an elevation, is the garden of Eden itself; nothing I ever saw can be compared to it. The town is considerable; but as the earthquake did not come on suddenly, as in the plain, not a life was lost there; but from a sickness, occasioned by hardships and fright, 52 have died since. I lodged in the barracks of a sensible gentleman of Mileto, Don Domenico Acquavetta, who is a principal proprietor of this town. He attended me the next day to the two tenements, called the Macini and Vaticano, mentioned in the former part of this letter, and which were said to have changed their situation by the earthquake. The fact is true, and easily accounted for. These tenements were situated in a valley surrounded by high grounds, and the surface of the earth, which has been removed, had been probably long undermined by little rivulets, which come from the mountains, and now are in full view on the bare spot the tenements had deserted. These rivulets have a sufficiently rapid course down the valley, to prove its not being a perfect level as was represented. I suppose the earthquake to have opened some depositions of rain-water in the clay hills which surround the valley, which water, mixed with the loose soil, taking its course suddenly through the undermined surface, lifting it up with the large olive and mulberry-trees, and a thatched cottage, floated the entire piece of ground, with all its vegetation, about a mile down

down the valley, where it now stands, with most of the trees erect. These two tenements may be about a mile long, and half a mile broad. I was shewn several deep cracks in this neighbourhood, not one above a foot in breadth; but which, I was credibly assured, had opened wide during the earthquake, and swallowed up an ox, and near an hundred goats, but no countrymen, as was reported. In the valley above mentioned I saw the same sort of hollows in the form of inverted cones, out of which, I was assured, that hot water and sand had been emitted with violence during the earthquakes as at Rosarno; but I could not find any one who could positively affirm that the water had been really hot, although the reports which government received affirm it. Some of the sand thrown out here with the water has a ferruginous appearance, and seems to have been acted upon by fire. I was told, that it had also, when fresh, a strong smell of sulphur, but I could not perceive it.

From hence I went through the same delightful country to the town of Polistene. To pass through so rich a country, and not see a single house standing on it, is most melancholy indeed; wherever a house stood, there you see a heap of ruins, and a poor barrack, with two or three miserable mourning figures sitting at the door, and here and there a maimed man, woman, or child, crawling upon crutches. Instead of a town, you see a confused heap of ruins, and round about them number of poor huts or barracks, and a larger one to serve as a church, with the church bells hanging upon a sort of low gibbet; every inhabitant with a doleful countenance, and wearing some token of having lost a parent.

I travelled four days in the plain, in the midst of such misery as cannot be described. The force of the earthquake was so great there, that all the inhabitants of the towns were buried

either alive or dead under the ruins of their houses in an instant. The town of Polistene was large, but ill situated between two rivers, subject to overflow. 2100 out of about 6000 lost their lives here the fatal 5th of February. The Marquis St. Giorgio, the baron of this country, whom I found here, was well employed in assisting his tenants. He had caused the streets of his ruined town to be cleared of rubbish, and had erected barracks on a healthy spot near it, for the remainder of his subjects, and on a good plan. He had also constructed barracks of a larger size for the silk-worms, which I found already at work in them. This prince's activity and generosity is most praise-worthy, and, as far as I have seen hitherto, he is without a rival. I observed, that the town of St. Giorgio, on a hill about two miles from Polistene, though rendered uninhabitable, was by no means levelled like the towns in the plain. There was a nunnery at Polistene; being curious to see the nuns that had escaped, I asked the marquis to shew me their barracks; but, it seems, only one out of twenty-three had been dug out of her cell alive, and she was fourscore years of age. After having dined with the marquis in his humble barrack, near the ruins of his very magnificent palace, I went through a fine wood of olive, and another of chefnut trees, to Casal Nuovo, and was shewn the spot on which stood the house of my unfortunate friend the princess Gerace Grimaldi, who with more than four thousand of her subjects lost her life by the sudden explosion of the 5th of February (for so it appears to have been) that reduced this town to atoms. I was told by some here, who had been dug out of the ruins, that they felt their houses fairly lifted up, without having had the least previous notice. In other towns some walls and parts of houses are standing; but here you neither distinguish street or house,

house, all lye in one confused heap of ruins. An inhabitant of Casal Nuovo told me, he was on a hill at the moment of the earthquake, overlooking the plain, when feeling the shock, and turning towards the plain, instead of the town, he saw in the place of it a thick cloud of white dust like smoke, the natural effect of the crushing of the buildings, and the mortar flying off.

From hence I went through the towns of Castellace and Milicusco (both in the same condition as Casal Nuovo) to Terra Nuova, situated in the same lovely plain, between two rivers, which, with the torrents from the mountains, have, in the course of ages, cut deep and wide chasms in the soft sandy clay soil of which the whole plain is composed. At Terra Nuova the ravine or chasm is not less than 500 feet deep, and three quarters of a mile broad. What causes a confusion in all the accounts of the phenomena produced by this earthquake in the plain, is the not having sufficiently explained the nature of the soil and situation. They tell you, that a town has been thrown a mile from the place where it stood, without mentioning a word of a ravine; that woods and corn-fields had been removed in the same manner, when in truth it is but upon a large scale, what we see every day upon a smaller, when pieces of the sides of hollow ways, having been undermined by rain waters, are detached into the bottom by their own weight. Here, from the great depth of the ravine, and the violent motion of the earth, two huge portions of the earth, on which a great part of the town stood, consisting of some hundreds of houses, were detached into the ravine, and nearly across it, about half a mile from the place where they stood; and what is most extraordinary, several of the inhabitants of those houses, who had taken this singular leap in them, were nevertheless dug out alive, and some unhurt. I spoke to one myself who had taken this extraordinary journey in his house, with his wife and a

maid-servant : neither he nor his maid-servant were hurt ; but he told me, his wife had been a little hurt, but was now nearly recovered. I happened to ask him, what hurt his wife had received ? His answer, though of a very serious nature, will nevertheless, I am sure, make you smile, Sir, as it did me. He said, she had both her legs and one arm broken, and that she had a fracture on her skull so that the brain was visible. It appears to me, that the Calabresi have more firmness than the Neapolitans ; and they really seem to bear their excessive present misfortune with a true philosophic patience. Of 1600 inhabitants at Terra Nuova, only 400 escaped alive. My guide there, who was a priest and physician, had been shut up in the ruins of his house by the first shock of the earthquake, and was blown out of it, and delivered by the succeeding shock, which followed the first immediately. There are many well-attested instances of the same having happened elsewhere in Calabria. In other parts of the plain situated near the ravine, and near the town of Terra Nuova, I saw many acres of land with trees and corn-fields that had been detached into the ravine, and often without having been overturned, so that the trees and crops were growing as well as if they had been planted there. Other such pieces were lying in the bottom, in an inclined situation ; and others again that had been quite overturned. In one place, two of these immense pieces of land having been detached opposite to one another, had filled the valley, and stopped the course of the river, the waters of which were forming a great lake : and this is the true state of what the accounts mention of mountains that had walked, and joined together, stopped the course of the river, and formed a lake. At the moment of the earthquake the river disappeared here, as at Rosarno, and returning soon after, overflowed the  
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bottom of the ravine about three feet in depth, so that the poor people that had been thrown with their houses into the ravine from the top of it, and had escaped with broken bones, were now in danger of being drowned. I was assured, that the water was salt, like that of the sea; but this circumstance seems to want confirmation. The same reason I have given for the sudden disappearing of the river Metauro at Rosarno will account for the like phenomenon here, and in every part of the country where the rivers dried up at the moment of the earthquake. The whole town of Mollochi di Sotto near Terra Nuova, was likewise detached into the ravine, and a vineyard of many acres near it lies in the bottom of the ravine as I saw in a perfect order, but in an inclined situation: there is a foot-path through this vineyard, which has a singular effect, considering its present impracticable situation. Some water mills, that were on the river, having been jammed between two such detached pieces as above described, were lifted up by them, and are now seen on an elevated situation, many feet above the level of the river. Without the proper explanations it is no wonder that such facts should appear miraculous. I observed in several parts of the plain, that the soil with timber trees and crops of corn, consisting of many acres, had sunk eight and ten feet below the level of the plain; and in others again I perceived it had risen as many. It is necessary to remember, that the soil of the plain is a clay mixed with sand, which is easily moulded into any shape. In the plain, near the spots from whence the above mentioned pieces had been detached into the ravine, there were several parallel cracks, so that had the violence of the shocks of the earthquake continued, these pieces also would have probably followed. I remarked constantly in all my journey, that near every ravine, or hollow way, the parts of the plain adjoining



were full of large parallel cracks. The earth rocking with violence from side to side, and having a support on one side only, accounts well for this circumstance. From Terra Nuova I went to Oppido. This city is situated on a mountain of a ferruginous sort of gritty stone, unlike the clay soil of its neighbourhood, and is surrounded by two rivers in a ravine deeper and broader than that of Terra Nuova. Instead of the mountain on which Oppido was situated having split in two, and by its fall on the rivers, stopped their course and formed great lakes, as we were told; it was (as at Terra Nuova) huge pieces of the plain on the edge of the ravine, that had been detached into it, nearly filled it up, and stopped the course of the rivers, the waters of which are now forming two great lakes. It is true, that part of the rock on which Oppido stood was detached with several houses into the ravine; but that is a trifling circumstance in comparison of the very great tracts of land, with large plantations of vines and olive-trees, which have been detached from one side of the ravine clear over to the other, though the distance is more than half a mile. It is well attested, that a countryman, who was ploughing his field in this neighbourhood with a pair of oxen, was transported with his field and team clear from one side of a ravine to the other, and that neither he nor his oxen were hurt. After what I have seen, I verily believe this may have happened. A large volume might be composed of the curious facts and accidents of this kind produced by the earthquakes in the valley; and, I suppose, many will be recorded in the account of the late formidable earthquakes, which the Academy of Naples intend to publish, the president having already sent into Calabria fifteen members, with draughtsmen in proportion, to collect the facts, and make drawings for the sole purpose of giving

giving a satisfactory and ample account of the late calamity to the publick ; but unless they attend, as I did, to the nature of the soil of the local where those accidents happened, their reports will generally meet with little credit, except from those who are professed dilettanti of miracles, and many such do certainly exist in this country. I met with a remarkable instance here of the degree of immediate distress to which the unfortunate inhabitants of the destroyed towns were reduced. Don Marcello Grillo, a gentleman of fortune, and of great landed property, having escaped from his house at Oppido, which was destroyed by the earthquake, and his money (no less than twelve thousand pieces of gold) having been buried under the ruins of it, remained several days without food or shelter during heavy rains, and was obliged to a hermit in the neighbourhood for the loan of a clean shirt. Having walked over the ruins of Oppido, I descended into the ravine, and examined carefully the whole of it. Here I saw, indeed, the wonderful force of the earthquake, which has produced exactly the same effects as I have described in the ravine of Terra Nuova, but on a scale infinitely greater. The enormous masses of the plain, detached from each side of the ravine, lie sometimes in confused heaps, forming real mountains, and having stopped the course of two rivers (one of which is very considerable) great lakes are already formed, and, if not assisted by nature or art, so as to give the rivers their due course, must infallibly be the cause of a general infection in the neighbourhood. Sometimes I met with a detached piece of the surface of the plain (of many acres in extent) with the large oaks and olive-trees, with lupins or corn under them, growing as well, and in as good order at the bottom of the ravine, as their companions, from whom they were separated, do on their native soil.

soil in the plain, at least 500 feet higher, and at the distance of about three quarters of a mile. I met with whole vineyards in the same order in the bottom, that had likewise taken the same journey. As the banks of the ravine, from whence these pieces came, are now bare and perpendicular, I perceived that the upper soil was a reddish earth, and the under one a sandy white clay, very compact, and like a soft stone; the impulse these huge masses received, either from the violent motion of the earth alone, or that assisted with the additional one of the volcanic exhalations set at liberty, seems to have acted with greater force on the lower and more compact stratum than on the upper cultivated crust: for I constantly observed, where these cultivated islands lay (for so they appeared to be on the barren bottom of the ravine) the under stratum of compact clay had been driven some hundred yards further, and lay in confused blocks, and, as I observed, many of those blocks were of a cubical form. The under soil having had a greater impulse, and leaving the upper in its flight, naturally accounts for the order in which the trees, vineyards, and vegetation, fell and remain at present in the bottom of the ravine. This curious fact, I thought, deserved to be recorded, but is not easily described by words. When the drawings and plans of the Academy are published, this account (imperfect as it is) may, perhaps, have its utility: had my time permitted, I would certainly have taken a draughtsman with me into Calabria. In another part of the bottom of the ravine there is a mountain composed of the same clay soil, and which was probably a piece of the plain detached by an earthquake at some former period; it is about 250 feet high, and about 400 feet diameter at its basis: this mountain, as is well attested, has travelled down the ravine near four miles, having been put in motion by

by the earthquake of the 5th of February. The abundance of rain which fell at that time, the great weight of the fresh detached pieces of the plain, which I saw heaped up at the back of it, the nature of the soil of which it is composed, and particularly its situation on a declivity, accounts well for this phenomenon; whereas the reports which came to Naples, of a mountain, in a perfect plain, having leaped four miles, had rather the appearance of a miracle. I found some single timber trees also with a lump of their native soil at the roots, standing upright in the bottom of the ravine, and which had been detached from the plain above mentioned. I observed also, that many confused heaps of the loose soil detached by the earthquake from the plains on each side of the ravine, had actually run like a volcanic lava (having probably been assisted by the heavy rain) and produced many effects greatly resembling those of lava during their course down a great part of the ravine. At Santa Cristina, in the neighbourhood of Oppido, the like phenomena have been exhibited, and the great force of the earthquake of the 5th of February seems to have been exerted on these parts and at Casal Nuova and Terra Nuova. The phenomena exhibited by the earthquakes in other parts of the plains of Calabria Ultra are of the same nature; but trifling in comparison of those I have been describing. The barracks erected for the remaining inhabitants of the ancient city of Oppido, now in ruins, are on a healthy spot, at about the distance of a mile from the old town, where I found the baron of this country, the Prince of Cariati, usefully employed in the assistance of his unfortunate subjects. He shewed me two girls, one of about sixteen years of age, who had remained eleven days without food under the ruins of a house at Oppido: she had a child of five or six months old in her arms, which

which died the fourth day. The girl gave me a clear account of her sufferings; having light through a small opening, she had kept an exact account of the number of days she had been buried. She did not seem to be in bad health, drinks freely, but has yet a difficulty in swallowing any thing solid. The other girl was about eleven years of age; she remained under the ruins six days only; but in so very confined and distressful a posture, that one of her hands, pressing against her cheek, had nearly worn a hole through it.

From Oppido I proceeded through the same beautiful country and ruined towns and villages to Seminara and Palmi. The houses of the former were not quite in such a ruined condition as those of the latter, whose situation is lower and nearer the sea. 1400 lives were lost at Palmi, and all the dead bodies have not been removed and burnt, as in most other parts I visited; for I saw myself two taken up whilst I was there, and I shall ever remember a melancholy figure of a woman in mourning, sitting upon the ruins of her house, her head reclined upon her hand and knee, and following with an anxious eager eye every stroke of the pick-axe of the labourers employed to clear away the rubbish, in hopes of recovering the corpse of a favourite child. This town was a great market for oil, of which there were upwards of 4000 barrels in the town at the time of its destruction, so that the barrels and jars being broken, a river of oil ran into the sea from it for many hours. The spilt oil mixed with the corn of the granaries, and the corrupted bodies, have had a sensible effect on the air. This I fear, as the heats increase, may prove fatal to the unfortunate remainder of the inhabitants of Palmi, who live in barracks near the ruined town. My guide told me, that he had been buried in the ruins of his house here by the first shock, and  
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that after the second, which followed immediately, he found himself sitting astride of a beam at least fifteen feet high in the air. I heard of many such extraordinary escapes in all parts of the plain, where the earthquake has exerted its greatest force.

From Palmi I proceeded through the beautiful woody mountains of Bagnara and Solano; noble timber oak trees on high rocks, narrow valleys with torrents in their bottoms, the road dangerous both on account of robbers and precipices. My two guards, instead of leading the way, as they had hitherto done, now separated and formed an advanced and a rear-guard. The narrow road was often interrupted by the fallen rocks and trees during the earthquakes, and obliged us to seek a new and still more dangerous road; but the Calabrese horses are really as sure-footed as goats. In the midst of one of these passes we felt a very smart shock of an earthquake, accompanied by a loud explosion, like that of springing a mine: fortunately for us it did not, as I expected, detach any rocks or trees from the high mountains that hung over our heads. After having passed the woods of Bagnara, Sinopoli, and Solano, I went through rich corn-fields and lawns, beautifully bounded with woods and scattered trees, like our finest parks, and which continue varying for some miles till you come upon the top of an open plain on a hill, commanding the whole Faro of Messina, the coast of Sicily as far as Catania, with Mount Etna rising proudly behind it, which altogether composed the finest view imaginable. From thence I descended a horrid rocky road to the Torre del Pezzolo, where there is a country-seat and a village belonging to the Princess of Bagnara. There I found, that an epidemical disorder had already manifested itself, as it probably will in many other parts of this glorious but unhappy country,

in proportion as the heats increase, owing to the hardships suffered, and the air having been spoiled by new-formed lakes. Several fishermen assured me, that during the earthquake of the 5th of February at night, the sand near the sea was hot, and that they saw fire issue from the earth in many parts. This circumstance has been often repeated to me in the plain; and my idea is, that the exhalations which issued during the violent commotions of the earth were full of electrical fire, just as the smoke of volcanoes is constantly observed to be during violent eruptions; for I saw no mark, in any part of my journey, of any volcanic matter having issued from the fissures of the earth; and I am convinced, that the whole damage has been done by exhalations and vapours only. The first shock felt at this place, as I was assured, was lateral, and then vorticose, and exceedingly violent; but what they call violent here, must have been nothing in comparison of what was felt in the plain of Casa Nuova, Polistene, Palmi, Terra Nuova, Oppido, &c. &c. where all agreed in assuring me, that the violence of the fatal shock of the 5th of February was instantaneous, without warning, and from the bottom upwards; and indeed in those places, where the mortality has been so great, and where nothing is to be seen but a confused heap of ruins, without distinction of either streets or houses, the violence of that shock is sufficiently confirmed. From this place to Reggio the road on each side is covered with villas and orange groves. I saw not one house levelled to the ground; but perceived that all had been damaged, and were abandoned; and that the inhabitants were universally retired to barracks in these beautiful groves of orange, mulberry, and fig-trees, of which there are many in the environs of Reggio. One that I visited, and which is reckoned the richest in all this part of Magna Grecia, is about a mile and a half from the town of Reggio, and what

is remarkable, belongs to a gentleman whose Christian name is Agamemnon. The beauty of the agrume (the general name of all kind of orange, lemon, cedrate, and bergamot-trees) is not to be described; the soil being sandy, the exposition warm, and command of water, a clear rivulet being introduced at pleasure in little channels to the foot of each tree, is the reason of the wonderful luxuriancy of these trees. Don Agamemnon assured me, it was a bad year when he did not gather from his garden (which is of no great extent) 170,000 lemons, 200,000 oranges (which I found as excellent as those of Malta), and bergamots enough to produce 200 quarts of the essence from their rinds. There is another singularity in these gardens, as I was assured, every fig-tree affords two crops of fruit annually; the first in June, the second in August. But to return to my subject, from which my attention was frequently called away by the extraordinary and uncommon beauty and fertility of this rich province; I arrived about sun-set at Reggio, which I found less damaged than I expected, though not a house in it is habitable or inhabited, and all the people live in barracks or tents; but after having been several days in the plain, where every building is levelled to the ground, a house with a roof, or a church with a steeple, was to me a new and refreshing object. The inhabitants of the whole country, that has been so severely afflicted with earthquakes, seem, however, to have so great a dread of going into a house, that when the earthquakes shall have ceased, I am persuaded, the greatest part of them will still continue to live in barracks. The barracks here (except some few that are even elegant) are ill constructed, as are in general throughout the country all barracks of towns that have been so little damaged as to allow the inhabitants to flatter themselves with a hope of being able to return to, and occupy,



their houses again, when the present calamity is at an end. Reggio has been roughly handled by the earthquakes, but is by no means destroyed. The archbishop, a sensible, active, and humane prelate, has distinguished himself from the beginning of the earthquakes to this day, having immediately disposed of all the superfluous ornaments of the churches, and of his own horses and furniture, for the sole relief of his distressed flock, with whom he cheerfully bears an equal share of every inconvenience and distress which such a calamity has naturally occasioned. Except in this instance, and very few others, indeed, I observed throughout my whole journey, a prevailing indolence, inactivity, and want of spirit, which is unfortunate, as such a heavy and general calamity can only be repaired by a disposition directly contrary to that which prevails; but as this government is indefatigable in its endeavours at remedying every present evil, and preventing such as may naturally be expected, it is to be hoped that the generous and wise dispositions lately made, will restore the energy that is wanting, and without which, one of the richest provinces in Europe is in danger of utter ruin. Silk and essence of bergamot, oranges and lemons, are the great articles of trade at Reggio. I am assured, that no less than 100,000 quarts of this essence is annually exported. The fruit, after the rind is taken off, is given to the cows and oxen; and the inhabitants of this town assure me, that the beef, at that season, has a strong and disagreeable flavour of bergamot. The worthy archbishop gave me an account of the earthquakes here in 1770 and 1780, which obliged the inhabitants (in number 16,400) to encamp or remain in barracks several months, without however having done any considerable damage to the town. I was assured here (where they have had such a long experience

science of earthquakes) that all animals and birds are in a greater or less degree much more sensible of an approaching shock of an earthquake than any human being; but that geese, above all, seem to be the soonest and most alarmed at the approach of a shock: if in the water, they quit it immediately, and there are no means of driving them into the water for some time after. The mortality here, by the late earthquake of the 5th of February, corresponds with the apparent degree of damage done to the town, and does not exceed 126. As it happened about noon, and came on gently, the people of Reggio had time to escape; whereas, as I have often remarked, the shock in the unhappy plain was as instantaneous as it was violent and destructive. Every building was levelled to the ground, and the mortality was general, and in proportion to the apparent destruction of the buildings. Reggio was destroyed by an earthquake before the Marston war, and having been rebuilt by Julius Cæsar was called Reggio Julio. Part of the wall still remains, and is called the Julian Tower; it is built of huge masses of stone without cement. Near St. Peruto, between Reggio and the Cape Spartivento, there are the remains of a foundery, his present Catholic Majesty, when King of Naples, having worked silver mines in that neighbourhood; which were soon abandoned, the profit not having answered the expence. There are some towns in the neighbourhood of Reggio that still retain the Greek language. About fifteen years ago, when I made the tour of Sicily, I landed at Spartivento in Calabria Ultra, and went to Bova, where I found that Greek was the only language in use in that district. On the 14th of May I left Reggio, and was obliged (the wind being contrary) to have my boats towed by oxen to the Punta del Pezzolo, opposite Messina, from whence the current wafted us with great expedition.

dition indeed into the port of Messina. The port and the town, in its half ruined state, by moon-light was strikingly picturesque. Certain it is, that the force of the earthquake (though very violent) was nothing at Messina and Reggio to what it was in the plain. I visited the town of Messina the next morning, and found, that all the beautiful front of what is called the Palazzata, which extended in very lofty uniform buildings, in the shape of a crescent, had been in some parts totally ruined, in others less; and that there were cracks in the earth of the quay, a part of which had sunk above a foot below the level of the sea. These cracks were probably occasioned by the horizontal motion of the earth in the same manner as the pieces of the plain were detached into the ravines at Oppido and Terra Nuova; for the sea at the edge of the quay is so very deep, that the largest ships can lie along-side; consequently the earth, in its violent commotion, wanting support on the side next the sea, began to crack and separate, and as where there is one crack there are generally others less considerable in parallel lines to the first, I suppose the great damage done to the houses nearest the quay has been owing to such cracks under their foundations. Many houses are still standing, and some little damaged, even in the lower part of Messina; but in the upper and more elevated situations, the earthquakes seem to have had scarcely any effect, as I particularly remarked. A strong instance of the force of the earthquake having been many degrees less here than in the plain of Calabria is, that the convent of Santa Barbara, and that called the Noviziato de' Gesuiti, both on an elevated situation, have not a crack in them, and that the clock of the latter has not been deranged in the least by the earthquakes that have afflicted this country for four months past, and which still continue in some degree.

degree. Besides, the mortality at Messina does not exceed 700 out of upwards of 30,000, the supposed population of this city at the time of the first earthquake, which circumstance is conclusive. I found, that some houses, nay a street or two, at Messina, were inhabited, and some shops open in them; but the generality of the inhabitants are in tents and barracks, which, having been placed in three or four different quarters, in fields and open spots near the town, but at a great distance one from the other, must be very inconvenient for a mercantile town; and unless great care is taken to keep the streets of the barracks, and the barracks themselves, clean, I fear that the unfortunate Messina will be doomed to suffer a fresh calamity from epidemical disorders, during the heat of summer. Indeed, many parts of the plain of Calabria seem to be in the same alarming situation, particularly owing to the lakes, which are forming from the course of rivers having been stopped; some of which, as I saw myself, were already green, and tending to putrefaction. I could not help remarking here, that the nuns, who likewise live in barracks, were constantly walking about, under the tuition of their confessor, and seemed gay, and to enjoy the liberty the earthquake had afforded them, and I made the same observation with respect to school-boys at Reggio; so that in my journal, which I wrote in haste, and from whence I have as hastily transcribed the imperfect account I send you, the remark stands thus: “*Earthquakes particularly pleasing to nuns and school-boys.*” Out of the cracks on the quay, it is said, that during the earthquakes fire had been seen to issue (as many I spoke with attested); but there are no visible signs of it, and I am persuaded it was no more than, as in Calabria, a vapour charged with electrical fire, or a kind of inflammable air. A curious circumstance happened here also,

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to prove that animals can remain long alive without food. Two mules belonging to the Duke of Belviso, remained under a heap of ruins, one of them twenty-two, and the other twenty-three days: they would not eat for some days, but drank water plentifully, and are now quite recovered. There are numberless instances of dogs remaining many days in the same situation; and a hen, belonging to the British vice-consul at Messina, that had been closely shut up under the ruins of his house, was taken out the twenty-second day, and is now recovered; it did not eat for some days, but drank freely; it was emaciated, and shewed little signs of life at first. From these instances, from those related before, of the girls at Oppido, and the hogs at Soriano, and from several others of the same kind, that have been related to me, but which being less remarkable I omit, one may conclude, that long fasting is always attended with great thirst, and total loss of appetite. From every inquiry I found, that the great shock of the 5th of February was from the bottom upwards, and not like the subsequent ones, which in general have been horizontal and vorticose. A circumstance worth remarking (and which was the same on the whole coast of the part of Calabria that had been most affected by the earthquakes) is, that a small fish called Cicirelli, resembling what we call in England white-bait, but of a greater size, and which usually lye at the bottom of the sea, buried in the sand, have been ever since the commencement of the earthquakes, and continue still to be, taken near the surface, and in such abundance, as to be the common food of the poorest sort of people; whereas, before the earthquakes, this fish was rare, and reckoned amongst the greatest delicacies. All fish, in general, have been taken in greater abundance, and with much greater facility, in those parts since they have been afflicted by earthquakes

earthquakes than before. I constantly asked every fisherman I met with on the coast of Sicily and Calabria, if this circumstance was true; and was as constantly answered in the affirmative; but with such emphasis, that it must have been very extraordinary. I suppose, that either the sand at the bottom of the sea may have been heated by the volcanic fire under it; or that the continual tremor of the earth has driven the fish out of their strong holds, just as an angler, when he wants a bait, obliges the worms to come out of the turf on a river side, by trampling on it with his feet, which motion never fails in its effect, as I have experienced very often myself. I found the citadel here had not received any material damage; but was in the same state as I had left it fifteen years ago. The Lazaret has some cracks in it, like those on the quay, and from a like cause. The port has not received any damage from the earthquakes. The officer who commanded in the citadel, and who was there during the earthquake, assured me, that on the fatal 5th of February, and the three following days, the sea, about a quarter of a mile from that fortress, rose and boiled in a most extraordinary manner, and with a most horrid and alarming noise, the water in the other parts of the Faro being perfectly calm. This seems to point out exhalations or eruptions from cracks at the bottom of the sea, which may very probably have happened during the violence of the earthquakes; all of which, I am convinced, have here a volcanic origin. On the 17th of May I left Messina, where I had been kindly and hospitably treated, and proceeded in my *Speronara* along the Sicilian coast to the point of the entrance of the Faro, where I went ashore, and found a priest who had been there the night between the 5th and 6th of February, when the great wave passed over that point, carried off boats, and above

twenty-four unhappy people, tearing up trees, and leaving some hundred weight of fish it had brought with it on the dry land. He told me, he had been himself covered with the wave, and with difficulty saved his life. He at first said the water was hot; but as I was curious to come at the truth of this fact, which would have concluded much, I asked him if he was very sure of it? and being pressed, it came to be no more than the water having been as warm as it usually is in summer. He said, the wave rose to a great height, and came on with noise, and such rapidity that it was impossible to escape. The tower on the point was half destroyed, and a poor priest that was in it lost his life. From hence I crossed over to Scilla. Having met with my friend the Padre Minasi, a Dominican friar, a worthy man and an able naturalist, who is a native of Scilla, and is actually employed by the Academy of Naples to give a description of the phenomena that have attended the earthquake in these parts, with his assistance on the spot, I perfectly understood the nature of the formidable wave that was said to have been boiling hot, and had certainly proved fatal to the baron of the country, the Prince of Scilla, who was swept off the shore into the sea by this wave, with 2473 of his unfortunate subjects. The following is the fact. The Prince of Scilla having remarked, that during the first horrid shock (which happened about noon the 5th of February) part of a rock near Scilla had been detached into the sea, and fearing that the rock of Scilla, on which his castle and town is situated, might also be detached, thought it safer to prepare boats, and retire to a little port or beach surrounded by rocks at the foot of the rock. The second shock of the earthquake, after midnight, detached a whole mountain, (much higher than that of Scilla, and partly calcareous,

and partly cretaceous), situated between the Torre del Cavallo and the rock of Scilla. This having fallen with violence into the sea (at that time perfectly calm) raised the fatal wave, which I have above described to have broken upon the neck of land, called the Punta del Faro, in the island of Sicily, with such fury, which returning with great noise and celerity directly upon the beach, where the prince and the unfortunate inhabitants of Scilla had taken refuge, either dashed them with their boats and richest effects against the rocks, or whirled them into the sea; those who had escaped the first and greatest wave were carried off by a second and third, which were less considerable, and immediately followed the first. I spoke to several men, women, and children here, who had been cruelly maimed, and some of whom had been carried into the sea by this unforeseen accident. Here, said one, my head was forced through the door of the cellar, which he shewed me was broken. There, said another, was I drove into a barrel. Then a woman would shew me her child, all over deep wounds from the stones and timber, &c. that were mixed with the water, and dashing about in this narrow port; but all assured me, they had not perceived the least symptom of heat in the water, though I dare say, Sir, you will read many well attested accounts of this water having been hot; of many dead bodies thrown up which appeared to have been parboiled by it; and of many living persons, who had evidently been scalded by this hot wave: so difficult is it to arrive at truth. Had I been satisfied with the first answer of the priest at the Punta del Faro, and set it down in my journal, who could have doubted but that this wave had been of hot water? Now that we are well acquainted with the cause of this fatal wave, we know it could not have been hot; but the testimony of so many unfortunate sufferers



from it, is decisive. A fact which I was told, and which was attested by many here, is very extraordinary indeed: a woman of Scilla, four months gone with child, was swept into the sea by the wave, and was taken up alive, floating on her back at some distance, nine hours after. She did not even miscarry, and is now perfectly well; and, had she not been gone up into the country, they would have shewn her to me. They told me, she had been used to swim, as do most of the women in this part of Calabria. Her anxiety and sufferings, however, had arrived at so great a pitch, that just at the time that the boat, which took her up, appeared, she was trying to force her head under water, to put a period to her miserable existence. The Padre Minafi told me another curious circumstance that happened in this neighbourhood, which to his knowledge was strictly true. A girl of about eighteen years of age, was buried under the ruins of a house six days, having had her foot, at the ankle, almost cut off by the edge of a barrel that fell upon it; the dust and mortar stopped the blood; she never had the assistance of a surgeon; but the foot of itself dropped off, and the wound is perfectly healed without any other assistance but that of nature. If of such extraordinary circumstances, and of hair-breadth escapes, an account was to be taken in all the destroyed towns of Calabria Ultra and Sicily, they would, as I said before, compose a large volume. I have only recorded a few of the most extraordinary, and such as I had from the most undoubted authority. In my way back to Naples (where I arrived the 23d of May) along the coast of the two Calabrias and the Principato Citra, I only went on shore at Tropea, Paula, and in the Bay of Palinurus. I found Tropea (beautifully situated on a rock overhanging the sea) but little damaged: however, all the inhabitants were in barracks.

barracks. At Paula the same. The fishermen here told me, they continued to take a great abundance of fish, as they had done ever since the commencement of the present calamity. At Tropea, the 15th of May, there was a severe shock of an earthquake, but of a very short duration. There were five shocks during my stay in Calabria and Sicily; three of them rather alarming: and at Messina, in the night-time, I constantly felt a little tremor of the earth, which has been observed by many of the Messinese. I am really ashamed, Sir, of sending such an unconnected hasty extract of my journal; but when I reflect, that unless I send it off directly, the Royal Society will be broken up for the summer season, and the subject will become stale before its next meeting; of two evils I prefer to chuse the least. Such rough draughts however (though ever so imperfect and incorrect) have, as in paintings, the merit of a first sketch, and a kind of spirit that is often lost when the picture is correctly finished. If you consider the fatigue and hurry of the journey I have just been taking; and that in the midst of the preparations for my other journey to England, which I propose to begin to-morrow, I have been writing this account, I shall hope then to be entitled to your indulgence for all its imperfections\*. But before I take my leave, I will just sum up the result of my observations in Calabria and Sicily, and give you my reasons for believing that the present earthquakes are occasioned by the operation of a volcano, the seat of which seems to lye deep, either under the bottom of the sea, between the island of Stromboli and the coast of Calabria, or under the parts of the plain towards Oppido and

\* *Queramus ergo quid sit quod terram ab infimo moveat, quid, &c. . . . Hæc ex quibus causis accidant digna res est excuti. See the whole passage very applicable here. SENECA, Nat. Quest. lib. VI. cap. 4.*

Terra Nuova. If on a map of Italy, and with your compass on the scale of Italian miles, you were to measure off 22, and then fixing your central point in the city of Oppido (which appeared to me to be the spot on which the earthquake had exerted its greatest force) form a circle (the radii of which will be, as I just said, 22 miles) you will then include all the towns, villages, that have been utterly ruined, and the spots where the greatest mortality has happened, and where there have been the most visible alterations on the face of the earth. Then extend your compass on the same scale to 72 miles, preserving the same center, and form another circle, you will include the whole of the country that has any mark of having been affected by the earthquake. I plainly observed a gradation in the damage done to the buildings, as also in the degree of mortality, in proportion as the countries were more or less distant from this supposed center of the evil. One circumstance I particularly remarked, if two towns were situated at an equal distance from this center, the one on a hill, the other on the plain, or in a bottom, the latter had always suffered greatly more by the shocks of the earthquakes than the former; a sufficient proof to me of the cause coming from beneath, as this must naturally have been productive of such an effect. And I have reason to believe, that the bottom of the sea, being still nearer the volcanic cause, would be found (could it be seen) to have suffered even more than the plain itself; but (as you will find in most of the accounts of the earthquake that are in the press, and which are numerous) the philosophers, who do not easily abandon their ancient systems, make the present earthquakes to proceed from the high mountains of the Apennines that divide Calabria Ultra, such as the Monte Dejo, Monte Caulone, and Aspramonte; I would ask them  
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this simple question, did the Æolian or Lipari islands (all which rose undoubtedly from the bottom of the sea by volcanic explosions at different, and perhaps very distant, periods) owe their birth to the Apeninnes in Calabria, or to veins of minerals in the bowels of the earth, and under the bottom of the sea? Stromboli an active volcano, and probably the youngest of those islands, is not above 50 miles from the parts of Calabria that have been most affected by the late earthquakes. The vertical shocks, or, in other words, those whose impulse was from the bottom upwards, have been the most destructive to the unhappy towns in the plain; did they proceed from Monte Dejo, Monte Caulone, or Alpramonte? In short, the idea I have of the present local earthquakes is, that they have been caused by the same kind of matter that gave birth to the Æolian or Lipari islands; that, perhaps, an opening may have been made at the bottom of the sea, and most probably between Stromboli and Calabria Ultra (for from that quarter all agree, that the subterraneous noises seem to have proceeded); and that the foundation of a new island or volcano may have been laid, though it may be ages, which to nature are but moments, before it is completed, and appears above the surface of the sea. Nature is ever active; but her actions are, in general, carried on so very slowly, as scarcely to be perceived by mortal eye, or recorded in the very short space of what we call history, let it be ever so ancient. Perhaps too, the whole destruction I have been describing may have proceeded simply from the exhalations of confined vapours, generated by the fermentation of such minerals as produce volcanoes, which have escaped where they met with the least resistance, and must naturally in a greater degree have affected the plain than the high and more solid grounds around it. When the account of the

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Royal Academy of Naples is published, with maps, plans, and drawings, of the curious spots I have described, this rude and imperfect account will, I flatter myself, be of use: without the help of plans and drawings you well know, Sir, the great difficulty there is in making one's self intelligible on such a subject. The inclosed letter, which I received whilst I was in Calabria Ultra, from the Marquis Ippolito, a gentleman of Catanzaro, and an able naturalist, will give you the particulars of the phenomena that have been produced by the late earthquakes in Calabria Citra, my time having permitted me to visit only a part of that province. I once more then crave your kind indulgence, and that of the members of our respectable Society, if you should think proper to communicate this hasty paper to them.

I have the honour to be, &c.

**XII.** *Account of the Earthquake which happened in Calabria, March 28, 1783. In a Letter from Count Francesco Ippolito to Sir William Hamilton, Knight of the Bath, F. R. S.; presented by Sir William Hamilton.*

Read July 10, 1783.

EXCELLENZA,

**Q**UESTA parte del nostro regno di Napoli, occupata un tempo da Bruzi, e da molte Greche colonie, che ora Calabria si appella, è stata sempre un funesto teatro del terribile fenomeno, che attualmente si osserva e si soffre. E pur recente la memoria de' tremuoti avvenuti nel 1638 e nel 1659 per i quali le due provincie della Calabria rimasero quasi interamente destrutte. Più fresca è l'idea che abbiamo del tremuoto accaduto nel 1743 in 44 che afflisse la nostra nazione per lungo spazio di tempo, mà senza ruina di città, ne perdita di cittadini. In Reggio, e ne Paesi all' intorno, i tremuoti quasi ogni anno si fanno sentire, che se riandar vogliamo le antiche memorie, noi troveremo nè secoli remoti, e della più oscura antichità, che tutta l'Italia, mà con ispezialità il nostro regno, e sopra tutto le nostre provincie, anno sofferte varie catastrofi avvenute per li fuochi volcanici, e per lo scoppio delle sotterranee accensione. Il culto religioso de Bruzi, per quanto dalle Storie rilevasi, vestito di tristezza, e di lutto, dimostra le funeste impressioni, che nell' animo di quei popoli, nostri antenati, faceva

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la spessa, ed orribile convulsione della terra. Ne poteva, ne può altrimenti avvenire in queste nostre regioni: essendo esse intersecate dalla Catena degli Appennini, nel seno de quali altro non trovasi, che Zolfi, Ferro, Carboni fossili, Olio petrolo, ed altre materie bituminose, e combustibili. Fra i molti minerali, di cui abbondano, di facile debbono farsi delle fermentazioni, ed accensioni sotterranee. Buon per noi, che abbiamo nelle nostre vicinanze varii vulcani, i quali servir possono di camini, per dar libero esito, e sfogo al fuoco che sotto à nostri piedi si accende!

Frà tanti famosi Tremuoti, che queste nostre regioni hanno sofferti, occupar non dee l'ultimo luogo quello, a cui ora soggiaccia, o che si riguardi la forza delle concussioni, e a di loro durata, o che si riguardino i cambiamenti prodotti nella superficie della terra e la ruina di tante città e ville, colla perdita di circa quarantamila abitanti.

Io dacché si sentì la prima orribile scossa de 5 Febbraio, cominciai a notare in un diario non solo le convulsioni, che giornalmente si sono dalla terra patite, mà eziandio le meteore tutte, che da quell' epoca in qua si sono giornalmente osservate nell' atmosfera. L'angustia del tempo non mi permette di trasferirlo, e mandarlo a vostra eccellenza, mà restringendo in poche parole quanto nel medesimo si contiene, posso assicurarla, che dal cennato di del 5 Febbraio infino a quest' ora le scosse sono state sempre spessissime, e quasi ogni giorno, or ondolando la terra, or foccotendosi, or movendosi vorticosamente in maniera, che ci pareva di essere allora sopra un naviglio agitato, e commosso da flutti tempestosi. In questa serie continuata di tremuoti non debbo lasciare di far osservare a vostra eccellenza, che le più notabili, e grandi accensioni sono seguite a di 5 Febbraio circa le ore 19½ d'Italia; a di 7 dello stesso mese verso le ore 20½,

a di 28 del sud. Febbraio circa le ore 8½ della notte seguente, e finalmente a di 28 di Marzo verso l'ora 1¼ della sera. Tutte le sudette quattro accensioni, per quanto puossi da fenomeni, e dagli effetti prodotti conjetturare, procedendo sempre dalla catena de Monti, che da Reggio a noi vengono, hanno prodotto quattro diverse esplosioni in quattro luoghi diversi della Calabria. Le tre prime furono in quella parte della provincia, in cui vostra Eccellenza di presente si trova, e per cui dovrà ella passare andando in Messina. Queste esplosioni diversi funesti effetti produssero. Città, e ville rovinate, monti scoppiati, grandi fenditure di terra, nuovi gorgghi di acque, antichi rivoli sprofondati e dispersi, fiumi trattenuti, terreni abbassati, piccioli monticelli nuovamente formati, piante sbarbicate e lungi trasportate dal primiero loro sito, terre vedute muoversi rotolando per lungo tratto, animali ed uomini dalla terra ingojati. Mà io mi astengo di farne a vostra eccellenza un minuto dettaglio. Ella colla propria ispezione, e colle relazioni de testimoni oculari di tali fenomeni, che costà potrà facilmente avere, se ne formerà una storia veridica; non posso però nè debbo lasciar di dirle, che fra tutt' i fenomeni in coteste parte accaduti, il più notabile è quello che ne' lidi di Scilla e di Bagnara avvenne. Quella parte di mare, che trabboccò straordinariamente in quelle maremme, ed ingojò più migliaja di uomini, che quivi si erano rifuggiati, si senti talmente calda, che scottò quei pochi, che dall' improvviso inondamento salvaronsi, siccome anche per bocca dell' eccellentissimo Sig. Vicario Generale ho saputo.

Io dunque mi tratterrò solo a narrarvi in brieve gli effetti dell' ultima esplosione de 28 d. Marzo, la quale, senza altro dovette avvenire da un' accensione fattasi nelle viscere della terra in questi nostri contorni, e precisamente ne' Monti, che vengono a traversare il collo della nostra Penisola formata dai due fiumi, Lameto, il



quale imbocca nel golfo di St. Eufemia, e Corace, che scorre nel mare Ionio, e proprio nel seno di Squillace. Tanto ne dimostrano i fenomeni da questa ultima concussione prodotti, de quali ho l'onore di fare un dettaglio a vostra eccellenza.

Questa scossa, come tutte le altre, dimostrò farsi per la direzione di Ponente-Lebeccio venendo a noi. Incominciò sulle prime ad ondolare la terra, quindi si concussè, finalmente si mosse vorticosamente in guisa, che molti stando all'impiedi, o caddero, o mal poterono reggersi sulle piante. Durò la terribile convulsione circa minuti dieci secondi, alla quale succedettero delle altre men forti, di minor durata, e di sola ondolazione, cosichè per tutta la notte, e per la metà del giorno seguente, prima per ogni 5 minuti, e poi per ogni quadrante di ora la terra si mosse.

Un terribile sotterraneo mugghito precedette alquanto, ed accompagnò la sopradetta concussione, la quale terminò finalmente con un tuono più intenso simile a quello che si farebbe da una mina, che scoppia. Simili tuoni accompagnarono costantemente non solo le scosse avvenute nella notte, e giorno seguente, mà quelle eziandio, che si sono fino a quest'ora sentite; che anzi si sono uditi alle volte de mugiti senza concussione alcuna di terra, e prima di 28 Marzo delle nostre vicine Montagne s'udivano strepiti e fragori, come tanti spari di Bombarde.

L'aere era coperto di nuvole, ed agitato da gagliardi venti occidentali, i quali poco prima, che seguìsse l'orrenda scossa, eranfi tutt' a un colpo calmati: ma immediatamente dopo di questa anche tutt' a un colpo risorsero, e poco dopo quietaronfi. Vi furono non dimeno nell'atmosfera in tutta quella notte delle spesse, e subitanee mutazioni, ora nuvoloso il Cielo mostrandosi, ora sereno, ora spirando un vento, ora un altro, sempre però dalla plaga riposta tra sud e Ovest.

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In quella notte medesima nelle vicinanze di questa città verso la marina, in cui si estese l'esplosione, nell'atto del tremuoto si videro delle fiammelle uscir dalla terra, in maniera, che vari contadini sorpresi da timore, si diedero alla fuga: e queste fiamme si videro precisamente sortire da un luogo, da cui giorni avanti un certo straordinario calore tramandavasi.

Dopo la gran concussione comparve nell'aria verso l'oriente una fiamma bislunga, albicante, simile al fuoco elettrico, che si sostenne per lo spazio di due ore in circa.

Al terribile scoppio vari Paesi, e città rimasero abbattute, e specialmente quelli, che sono situati nelle vicinanze, e nel collo stesso della nostra Penisola, procedendo da Tiriolo fino al fiume Angitola, e che niun danno aveano da precedenti tremuoti sofferto. Curinga, Majda, Cortale, Girifalco, Borgia, St. Floro, Settingiano, Marcellinara, Tiriolo, ed altri Paesi di minor conto furono pressoché interamente distrutti mà con perdita di poca gente, se non che in Majda, Cortale, e Borgia più centinaia rimasero vittime delle ruine.

Quelli stessi effetti che i primi tremuoti cagionato aveano in quei luoghi ove trovasi vostra eccellenza furono anche prodotti dall'ultimo tremuoto in queste nostre Contrade. Furono molte Colline ove aperte, ove spianate. Molte aperture incontrasfatte nella terra per tutta la superficie riposta tra le due valli de' fiumi Corace, e Lameto procedendo verso il fiume Angitola. Da molte di queste aperture sgorgò per più ore acqua copiosa tramandata, o dalle sotterranee concamerazioni, o dall'istesso fiumi, vicino a quali venne a svenarsi la terra. Da una di esse fatta nel territorio di Borgia, distante circa un miglio dal mare, uscì copiosamente dell'acqua salza per più giorni, la quale sgorgando imitava i vari moti dell'onda stessa marina. Dalle aperture fattesi nelle pianure di Majda sgorgò dell'acqua calda ;  
mà.

ma dir non so, se fosse stata termale, o pur riscaldata dalla stessa sotterranea accensione.

E ancor d'avvertirsi, che da quell'istesse fessure, dalle quali apparve sgorgar dell' acqua, si tramandò dell' arena tenuissima, ove biggia, ove gialliccia, ove biancastra, la quale per la sua estrema sottigliezza sembra quasi un Sabbione. Di sì fatte arene ho io avuta la sola biggia, nelle quale vedesi chiaramente frammischiata parte di ferro.

Si è in oltre osservato, che nelle parti arenose, ove si è fatta l'esplosione, tratto tratto s'incontrano delle aperture in forma di cono inverso, dalle quali è anche uscita dell' acqua; lochè par che dimostri essere quindi scappato un fiocco di fuoco elettrico. Fessure di tal forma s'incontrano specialmente lungo le rive del fiume Lameto dalla sua imboccatura in quà per più miglia.

Trà fenomeni, che precedettero, e seguirono il tremuoto de 28 Marzo sono degni di osservazione i due seguenti. Nel giorno stesso in Majda l'acqua sorgiva di un pozzo, che prima bevasi, si trovò infetta di un disgustevole sapore sulfureo, che anche proibiva il sutarla. In Catanzaro all' incontro dopo il tremuoto sudetto l'acqua di un pozzo, che prima non potea usarsi, perchè di un sapor calcinoso, si è poscia resa purissima, sicchè bevesi felicemente. In Majda stessa nel tremuoto de 28 molte fontane si disseccarono, come è avvenuto ben anche in altri luoghi: ma molte altre ne sono scaturite in varie parti, ove non erano altra volta comparse; che anzi sono comparse delle nuove sorgenti minerali, di cui non vi era vestigio, come è avvenuto in Cropani Paese del Marchesato. Ma ordinariamente le fontane si sono rese più gonfie, e più copiose, gittando acqua in un maggior volume del solito.

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Si sono anche osservate le acque delle fontane rese torbide, e di colore, ove bianchiccio, ed ove giallastro, secondo la natura de' terreni per dove scorrevano.

Varie elevazioni di terra seguite sono per lo tremuoto medesimo; la più notabile è quella che avvenne nel letto del fiume di Borgia, ove alzata si vede una nuova collinetta, alta circa tredici palmi, larga venti nella sua base, e lunga circa ducento palmi. Finalmente nelle vicinanze del fiume Lameto, e precisamente nell' distretto della terra detta di Amato, tutto sbarbicato dal tremuoto, vedesi un oliveto, e vedesi ancora, che la superficie di tal fondo ne fù rivoltata vorticosamente, come in coteste contrade in varii luoghi avvenne nè primi tremuoti.

Questi sono i più notabili fenomeni avvenuti in questa nostra reggione per lo fatale tremuoto de 28 Marzo, e che giunte sono fin ora a mia notizia. Mi credo però nell' obbligo di soggiungere a vostra eccellenza, che questa funesta catastrofe dell' afflitta nostra provincia, fù preceduta da grandi, ed insoliti giacci avvenuti nell' inverno del 1782; da straordinaria siccità, e da insosfribili calori nella primavera ed estate del medesimo anno; da grandi, copiose, e continue piogge cadute nell' autunno, e continuate per tutto Gennaio dell' anno corrente. Fra queste dirotte piogge, non iscoppiò quasi mai tuono ne fulmine; ed in questa città, in cui sogliono sempre soffiar con gagliardia i venti, per tutto questo tratto di tempo, o di raro, o leggermente si fecero sentire; mà poi nel principiar del tremuoto, disfierraronsi con massimo impeto, e furore, accompagnati ora da pioggia, ora da gragnuola. Molto tempo prima che si scotesse la terra, videfi il mare gonfio ed elevato, senza che vi fusse vento, onde venissero le sue onde agitate, in guisa che i pescatori stessi non ardivano di entrarvi. I nostri vulcani per  
quanto

quanto asseverantemente mi si è riferito, per lungo spazio prima non aveano affatto eruttato. Mà l'Etna eruttò nè primi tremuoti, e Stromboli fece vedere il suo fuoco negli ultimi. Or voglia il Cielo, che la terra rimettasi al fine nella sua perduta fermezza, ed abbia a tornare a noi il perduto equilibrio tanto nell' ordine fisico, che morale. In tanto con profondo rispetto mi rassegno.

Di vostra eccellenza, &c.

XIII. *Account of the Black Canker Caterpillar, which destroys the Turnips in Norfolk. By William Marshall, Esq. in a Letter to Charles Morton, M. D. F. R. S.*

Read February 8, 1783.

S I R,

Gunton, near Aylsham, Norfolk,  
August 22, 1782.

A FEW months after you did me the honour of presenting my minutes of agriculture to the British Museum, I came down into Norfolk, as agent to Sir HARBORD HARBORD.

To a person intelligent in matters of agriculture it would be superfluous to say, that Norfolk is celebrated for good husbandmen; or that the turnip crop is the basis of the Norfolk husbandry. If a Norfolk farmer loses his crop of turnips, his farm is injured for several succeeding years; for it is not only the loss of the immediate profit, which would otherwise have arisen to him from his bullocks, but his land is deprived of the consequent manure and trampling (esteemed highly beneficial to the light lands of this county) on which his future crops of corn are essentially dependant.

Among the numerous enemies to which turnips are liable, none have proved more fatal here than the Black Canker (a species of Caterpillar) which in some years have been so numerous as to cut off the farmer's hopes in a few days. In other years, however, the damage has been little, and in others nothing. About twenty years ago the whole country

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was nearly stripped; and this year it has been subjected to a similar fate. Many thousands of acres, upon which a fairer prospect for a crop of turnips has not been seen for many years, have been plowed up; and as, from the season being now far spent, little profit can be expected from a second sowing; the loss to the farmers; individually, will be very considerable, and to the county immense.

It was observed in the canker-year above mentioned, that, prior to the appearance of the caterpillars, great numbers of yellow flies were seen busy among the turnip plants; and it was then suspected, that the canker was the caterpillar state of the yellow fly; and since that time it has been remarked, that cankers have regularly followed the appearance of these flies. From their more frequently appearing on the sea-coast, and from the vast quantities which have, I believe, at different times, been observed on the beach washed up by the tide, it has been a received opinion among the farmers, that they are not natives of this country, but come across the ocean, and observations this year greatly corroborate the idea. Fishermen upon the eastern coast declare, that they actually saw them arrive in cloud-like flights; and from the testimony of many, it seems to be an indisputable fact, that they first made their appearance on the eastern coast; and, moreover, that on their first being observed, they lay upon and near the cliffs so thick and so languid, that they might have been collected into heaps, lying, it is said, in some places two inches thick. From thence they proceeded into the country, and even at the distance of three or four miles from the coast they were seen in multitudes resembling swarms of bees. About ten days after the appearance of the flies, the young caterpillars were first observed on the under sides of the leaves of the turnips, and in  
seven

seven or eight days more, the entire plants, except the strongest fibres, were eaten up. A border under the hedge was regularly spared until the body of the inclosure was finished; but this done, the border was soon stripped, and the gateway, and even the roads have been seen covered with caterpillars travelling in quest of a fresh supply of turnips; for the grasses, and indeed every plant, except the turnip and the charlock (*sinapis arvensis*) they entirely neglect, and even die at their roots, without attempting to feed upon them. This destruction has not been confined within a few miles of the eastern coast, but has reached, more or less, into the very center of the county. The mischief, however, in the western parts of Norfolk, and even on the north coast, has been less general; but I am afraid it may be said, with a great deal of truth, that one half of the turnips in the county have been cut off by this voracious animal.

A circumstance so discouraging to industry, and injurious to the public at large, will, I flatter myself, Sir, be thought a sufficient apology for my troubling you with a relation of it, and for my taking the liberty of sending you a male and a female fly, also one of the animals in its caterpillar, and one which is in its chrysalis state, for your inspection, hoping that the public may become acquainted with the means of preventing in future so great a calamity.

Left the flies may become disfigured in travelling, it may be prudent to say, that their wings are four; that their antennæ are clubbed, and about one-third of the length of their body, each being composed of nine joints, namely, two next the head, above which two there is a joint somewhat longer than the rest, and above this six more joints, similar to the two below; that near the point of the tail of the female there is a



black speck, outwardly fringed with hair; but which, opening longitudinally, appears to be the end of a case, containing a delicate point or sting (about one-twentieth of an inch in length) which on a cursory view appears to be a simple lanceolated instrument, with a strong line passing down the middle, and ferrated at its edges; but, on a closer inspection, and by agitating it strongly with the point of a needle, it separates into three one-edged instruments, hanger-like as to their general form, with a spiral line or wrinkle winding from the point to the base, making ten or twelve revolutions, which line, passing over their edges, gives them some appearance of being ferrated.

By the help of these instruments, I apprehend, the female deposits her eggs in the edge of the turnip-leaf (or sometimes, perhaps, in the nerves or ribs on the under surface of the leaf); thus far I can say, and I think with a considerable degree of certainty, that having put some fresh turnip leaves into a glass containing several of the male and female flies, I perceived (by the means of a simple magnifier) that one of the females, after examining attentively the edge of the leaf, and finding a part which appeared to me to have been bitten, unsheathed her instruments, insinuated them into the edge of the leaf, and having forced them asunder so as to open a pipe or channel between them, placed her pubes (the situation of which from repeated and almost incessant copulations I had been able to ascertain precisely, and to the lower part of which these instruments seem to be fixed) to the orifice, and having remained a few seconds in that posture, deliberately drew out the instruments (which the transparency of the leaf held against a strong light afforded me an opportunity of seeing very plainly) and proceeded to search for another convenient place for her purpose.

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The caterpillar has twenty feet (six of its legs being of considerable length, the other fourteen very short) and in its first stage is of a jetty black, smooth as to a privation of hair, but covered with innumerable wrinkles. Having acquired its full size, it fixes its hinder parts firmly to the leaf of a turnip, or any other substance, and breaking its outer coat or slough near the head, crawls out, leaving the skin fixed to the leaf, &c. The under coat, which it now appears in, is of a blueish or lead colour, and the caterpillar is evidently diminished in its size. In every respect it is the same animal as before, and continues to feed on the turnips for some days longer: it then entirely leaves off eating, and becomes covered with a dewy moisture, which seems to exude from it in great abundance, and appearing to be of a glutinous nature, retains any loose or pliant substance which happens to come in contact with it, and by this means alone seems to form its chrysalis coat. One I find laid up in the fold of a withered turnip leaf (that which I have the honour of inclosing you) was, among six others, formed by putting common garden mould to them while they were in the exsudatory state above described.

From the generic characters of the fly I conclude it to be a *Tenthredo* of HILL; but whether that voluminous author be sufficiently accurate; or whether, from being an almost entire stranger to natural history, I may, or may not, sufficiently understand my book, I must beg leave to submit to your superior knowledge of the subject.

I am endeavouring to extend my observations on these insects, and am making some experiments concerning them, the result of which I should be extremely happy in being permitted to communicate to you; and it may be proper to add here, that

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I should not have taken the liberty of troubling you, prematurely with this letter, had I not luckily met with an opportunity of procuring some live flies (which are now become very scarce) ; and I flatter myself they will come to your hands in a perfect state.

I am, with the greatest respect, &c.

XIV. *A Letter from Mr. Edward Nairne, F. R. S. to Sir Joseph Banks, Bart. P. R. S. containing an Account of Wire being shortened by Lightning.*

Read February 3, 1783.

S I R,

**I**N the Philosophical Transactions for the year 1780, vol. LXX. are printed some experiments of mine, shewing the method of shortening of wire by the effect of electricity. I have since met with a similar circumstance produced by lightning; and, if the following account should meet with your approbation, should be happy to have it communicated to the Society.

On the 18th of June, 1782, Mr. PARKER's house at Stoke Newington was struck by lightning, between two and three o'clock in the afternoon. The lightning passed down the leaden pipe without side the house, which pipe did not reach to the ground by about ten feet. Here the lightning struck from one of the nails which fastened this leaden pipe to the wall to the end of a crank iron that was drove in the wall opposite it, withinside the room, and to which was fastened the wire of a night-bolt, rather thicker than usual. This wire was so very loose before the accident happened, that the bolt could not be raised by the handle at the bed-side, so that they were obliged every night to take hold of the bolt itself to lift it up to fasten the door; but on the night after the accident had happened, they, on going to bed, went to raise the bolt up as usual, to secure their chamber-door, when, to their  
great

great surprize, they found the bolt drawn up; and on trying to pull it down, they could not with all their strength. It being a particular acquaintance of mine, they sent for me. I went the next day, and not only found the bolt drawn up, but the wire, which they told me before was very loose, and much bent, was drawn very straight, and so tight, that when struck it produced a musical tone. The wire was judged to be shortened several inches; for, had the wire before the accident been straight, it must have shortened it above two inches to have drawn the bolt up.

The whole length of the wire from the bed-side to the bolt was about thirty feet; but the part of the wire on which the lightning passed was about fifteen feet.

Near the crank iron that was directly over the bolt were two wires, which passed through the wainscot to a single one belonging to an alarm. The lightning passed these two wires, without damaging them; but the single one was partly dispersed into smoke, blackening all the wainscot near it; also a great deal was melted into globules, which globules we found by a magnet.

This was the first instance (and, I must own, it agreeably surprized me) that I had ever met with of wire being contracted or shortened by the effect of lightning, though I have now not the least doubt, but that it is always the case; and that is the reason that we find them mostly broke where the lightning has passed, if it does not melt them. I have often shortened wire by electricity, an account of which I gave to the Royal Society as before mentioned.

I have brought a piece of wire belonging to the night-bolt, and also some of the globules, for the inspection of the Society.

I have the honour to be, &c.

P O S T-

## P O S T S C R I P T.

Being desirous of knowing whether the lightning had any ways altered the property of the iron by melting it into globules, I applied to the hon. Mr. CAVENDISH, who very obligingly tried them with different acids, and found that they scarcely shewed any signs of effervescence even when heated over the fire. He next tried some iron filings, which he put to some of the same acid; these not only caused an effervescence, but were intirely dissolved.

He also tried the pieces of steel struck off by striking a light, which being separated by a magnet from the pieces of flint effervesced with the same acids, and dissolved almost intirely, only half a grain being left out of eighteen, and these consisted principally of those parts that were melted in globules.

XV. *An Account of Ambergrise, by Dr. Schwediawer :  
presented by Sir Joseph Banks, P. R. S.*

Read February 13, 1783.

**A**MBERGRISE, or properly speaking *Grey Amber*, is a solid, opaque, inflammable substance, of a white grey, sometimes of a blackish colour, which melted or inflamed yields a peculiar smell, agreeable to most persons, but disagreeable to others.

As it occurs in the shops, it varies in its consistence, according as it has been exposed to a warmer or colder air. It is a hard brittle substance, yet not so hard as to admit a polish; nor has it, like *succinum*, a polished appearance or transparency. On scraping it with a knife into powder, part of it adheres to the cold steel like wax; so it does also to the teeth, if masticated; it yields also the impression of the nail; it has no peculiar but rather an earthy taste when chewed.

It has in its natural state a peculiar strong smell. The older it grows the more it seems to become agreeable. This smell is rendered more sensible by rubbing it with the fingers, or by burning or melting it.

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It melts in a moderate degree of heat into a blackish thick oil, and then smoaks, skums, and flies by degrees entirely off, without leaving any coal behind; so it does likewise when put upon any heated metal, leaving only a black spot upon it: when the metal is red-hot, it melts and inflames instantaneously, smoaks strongly, and flies speedily off, without leaving the least mark behind. When brought near a burning candle it catches fire immediately, and burns with a clear bright flame till it is consumed. A red-hot needle easily penetrates through its substance, a blackish oil then exsudes, but no part of it seems to adhere to the needle; the needle, however, feels afterwards as if it had been put into wax.

It is so light, that it swims not only upon the sea, but also on the surface of fresh water.

Its colour is white grey, or yellowish, or blackish, the first of which is esteemed the best. All ambergrise, when kept for a certain time, is covered with a kind of white grey dust like chocolate. When broken it appears to be of a granulated texture; and in some pieces it seems to be laid on in strata.

It feels rather rough when first touched, but, when rubbed with the finger, it feels like hard soap, or rather like that kind of stone which the mineralogists call *Smectis*.

It is found swimming upon the sea, or the sea-coast, or in the sand near the sea-coast; especially in the Atlantic Ocean, on the sea-coast of Brasil, and that of Madagascar; on the coast of Africa, of the East Indies, China, Japan, and the Molucca Islands; but most of the ambergrise which is brought to England comes from the Bahama Islands, from Providence, &c. where it is found on the coast. It is also sometimes found in the abdomen of whales by the whale-fishermen, always in lumps of various shapes and sizes, weighing from half an ounce to an hundred



and more pounds. The piece, which the Dutch East India Company bought from the King of Tydor, weighed 182 pounds. An American fisherman from Antigua found some years ago, about 52 leagues south-east from the Windward Islands, a piece of ambergrise in a whale, which weighed about 130 pounds, and sold for five hundred pounds sterling.

We are told by all writers on ambergrise, that sometimes claws and beaks of birds, feathers of birds, parts of vegetables, shells, fish, and bones of fish, are found in the middle of it, or variously mixed with it; but of a very large quantity of pieces which I have seen, and which I have carefully examined, I have found none that contained any such thing, though I do not deny, that such substances may sometimes be found in it; but the circumstance which to me seems to be the most remarkable, is, that in all the pieces of ambergrise of any considerable size, whether found on the sea, or in the whale, which I have seen, I have constantly found a considerable quantity of black spots, which, after the most careful examination, appear to be the beaks of the *Sepia Octopodia*. These beaks seem to be the substances which have hitherto been always mistaken for claws or beaks of birds, or for shells.

Having collected a pretty large quantity of them, I beg leave to present to the Royal Society some specimens, in which the whole structure of these beaks is extremely well preserved. They are accompanied by a beak which Sir JOSEPH BANKS permitted me to take from a cuttle fish in his collection, so that any gentleman, who will be at the pains to compare them together, will be enabled to convince himself of the truth of what I have advanced.

The presence of these beaks in ambergrise proves evidently, that all ambergrise containing them is in its origin, or must have

have been once, of a very soft or liquid nature, as otherwise those beaks could not so constantly be intermixed with it throughout its whole substance.

In order to come now more closely to the point proposed (*viz.* to determine the origin and nature of ambergrise), let us recollect some of the principal facts relative to its natural history.

That ambergrise is found either upon the sea and sea-coast, or in the bowels of whales, is a matter of fact, which, I believe, is universally credited. But it has never been examined into and determined, whether the ambergrise found upon the sea and sea-coast is the same as that found in the whale, or whether they are different from one another? Whether that found on the sea or sea-coast has some properties, or constituent parts, which that found in the whale has not? And lastly, Whether that found in the whale is superior or inferior in its qualities and value to the former?

It is likewise a matter of consequence to know, Whether ambergrise is found in all kinds of whales, or only in a particular species of them? Whether it is constantly and always to be met with in those animals? And, if so, in what part of their body it is to be found?

It is further a matter of enquiry, Whether, on those coasts where ambergrise is found, there are also constantly, or only accidentally, whales to be met with? Whether ambergrise is found there because whales frequent those seas, or rather whether whales are there because ambergrise is to be met with there?

It ought likewise to be investigated, Whether all ambergrise is of the same mineral or animal origin? If of the former, whether it is swallowed by the whale, and digested or changed in some manner in its stomach? Or, if of the latter, whether  
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it is an animal production generated in the stomach as a kind of bezoar, according to CLUSIUS; or secreted in a peculiar bag, according to DUDLEY, &c.? Or lastly, Whether it is, according to KÆMPFER, the excrement or dung of the whale?

All these questions ought to be discussed and precisely answered, before we can determine any thing with certainty about the origin of ambergrise.

In order to clear up this point, we must apply to the persons who are employed in procuring and selling ambergrise. This is what I have had an opportunity of doing through the kindness of Sir JOSEPH BANKS, baronet, whose zeal to promote every part of useful knowledge is so generally known and acknowledged by the public. Sir JOSEPH very obligingly procured me the acquaintance of two captains of ships, men of good sense and veracity, who offered to tell me every thing they knew about the matter, and who began with assuring me that they would speak only as to what they themselves had seen, and that not once only, but repeatedly, as they have both of them been employed many years in what is called in England the South Fishery. I have since had an opportunity of conversing on the same subject with an intelligent native of New England, who before the present war broke out was employed for several years in the spermaceti-whale fishery from Boston. From these three persons I have collected the following facts:

Ambergrise is sometimes found in the belly of the whale, but in that particular species only which is called the spermaceti whale, and which from its description and delineation appears to be the *Phyfeter Macrocephalus* Linnæi.

The New England fishermen, according to their account, have long known that ambergrise is to be found in the spermaceti whale; and they are so convinced of this fact, that when-  
ever

ever they hear of a place where ambergrise is found, they always conclude that the seas in that part are frequented by this species of whale. It was for this reason that a gentleman at Boston, upon hearing several years ago that ambergrise was frequently found on the coast of Madagascar, immediately proposed a plan for a spermaceti-whale fishery in that part of the world. And the two persons I conversed with on this subject have not the least doubt but he would have succeeded in the attempt, had not the East India Company frustrated the project, by pretending, that as it was in their territory the right of fishery could belong only to them. This was all they did, however, as the plan itself they never adopted.

The persons who are employed in the spermaceti-whale fishery, confine their views to the *Phyfeter Macrocephalus*. They look for ambergrise in all the spermaceti-whales they catch, but it seldom happens that they find any. Whenever they hook a spermaceti-whale, they observe, that it constantly not only vomits up whatever it has in its stomach, but also generally discharges its fæces at the same time; and if this latter circumstance takes place, they are generally disappointed in finding ambergrise in its belly. But whenever they discover a spermaceti-whale, male or female, which seems torpid and sickly, they are always pretty sure to find ambergrise, as the whale in this state seldom voids its fœces upon being hooked. They likewise generally meet with it in the dead spermaceti-whales which they sometimes find floating on the sea. It is observed also, that the whale, in which they find ambergrise, often has a morbid protuberance; or, as they express it, a kind of gathering in the lower part of its belly, in which, if cut open, ambergrise is found. It is observed, that all these whales, in whose bowels am-

bergrise

bergrise is found, seem not only torpid and sick, but are also constantly leaner than others; so that, if we may judge from the constant union of these two circumstances, it would seem that a larger collection of ambergrise in the belly of the whale is a source of disease, and probably sometimes the cause of its death. As soon as they hook a whale of this description, torpid, sickly, emaciated, or one that does not dung on being hooked, they immediately either cut up the above-mentioned protuberance, if there be any, or they rip open its bowels from the orifice of the anus, and find the ambergrise, sometimes in one sometimes in different lumps of generally from three to twelve and more inches in diameter, and from one pound to twenty or thirty pounds in weight, at the distance of two, but most frequently of about six or seven feet from the anus, and never higher up in the intestinal canal, which, according to their description, is, in all probability, the intestinum cœcum, hitherto mistaken for a peculiar bag made by nature for the secretion and collection of this singular substance. That the part they cut open to come at the ambergrise is no other than the intestinal canal is certain, because they constantly begin their incision at the anus, and find the cavity every where filled with the faces of the whale, which from their colour and smell is impossible for them to mistake. The ambergrise found in the intestinal canal is not so hard as that which is found on the sea or sea-coast, but soon grows hard in the air: when first taken out it has nearly the same colour, and the same disagreeable smell, though not so strong, as the more liquid dung of the whale has; but, on exposing it to the air, it by degrees not only grows greyish, and its surface is covered with a greyish dust like old chocolate, but it also loses its disagreeable smell, and, when kept for a certain length of time, acquires the peculiar odour which is so agreeable to most people.

The gentlemen I conversed with confessed, that if they knew not from experience that ambergrise thus found will in time acquire the above-mentioned qualities, they would by no means be able to distinguish ambergrise from hard indurated faeces. This is so true, that whenever a whale voids its faeces upon being hooked, they look carefully to see if they cannot discover among the more liquid excrements (of which the whale discharges several barrels) some pieces floating on the sea, of a more compact substance than the rest; these they take up and wash, knowing them to be ambergrise.

From this account it appears therefore clearly, that CLUSIUS is quite wrong in asserting that ambergrise is a phlegmatic recrement, or indurated undigestible part of the food collected and found in the stomach of the whale, in the same manner as the bezoars are found in the stomach of other animals. It appears further, that what DUDLEY says, in *Phil. Trans.* vol. XXIII. from an account he received from a whale-fisherman, one Mr. ATKINS, of Boston in New England, who was one of the first who went out a fishing for the spermaceti-whale about the year 1720, viz. that the ambergrise found in whales is a kind of animal production like musk and castoreum, &c. secreted and collected in a peculiar bag or bladder, which is furnished with an excretory duct or canal, the spout of which runs tapering into and through the length of the penis; and that this bag, which lies just over the testicles, is almost full of a deep orange-coloured liquor, not quite so thick as oil, of the same smell as the balls of ambergrise, which float and swim loose in it; which colour and liquor may also be found in the canal of the penis; and that therefore ambergrise is never to be found in any female, but in the male only, is equally destitute of truth. The asser-

tions are not only destitute of truth, but also contrary to the laws of the animal economy; for, in the first place, the gentlemen whom I consulted have repeatedly found ambergris in males as well as females; they think, however, to have remarked, that the ambergris found in females is never in such large pieces, or of so good a quality, as that which is found in males. 2dly, No man, who has the least knowledge in anatomy and the animal economy, will ever believe that organised bodies, such as the beaks of the Sepia, which are so constantly found in ambergris taken out of the whale, can have been absorbed from the intestines by the lacteals or lymphatics, and collected with the ambergris in the bag mentioned by ATKINS and DUDLEY. If either of these persons had known the nature of these substances, and had had the least knowledge of the different secretions in animal bodies, they would certainly never have ventured to give such a description as a true one.

KEMPFER, who has given us so many other faithful accounts in Natural History, seems to come nearer the truth with regard to the origin of ambergris, when he says, that it is the dung of the whale; and that the Japanese, for this reason, call it, *Kufura no furu*, i. e. Whale's Dung; but this relation, though founded on observation, has never obtained credit, and has been considered rather as a fabulous story, with which the Japanese imposed upon him, who had himself no direct observation to prove the fact.

This matter therefore remained a subject of great doubt, and it was generally thought to be more probable, that ambergris, after having been swallowed, and somehow or other changed in the stomach and bowels of the whale, was found among its excrements. But in order to discuss this matter fully, and

bring

bring it nearer to that degree of certainty which I proposed at the beginning of this paper, it will now be proper to examine the principal question, Whether all ambergrise is generated in the bowels of the whale, or whether it is simply an extraneous substance taken in with the food? In order to elucidate this matter, it will be necessary to resolve the following questions:

1st. Whether there is any material difference between ambergrise found upon the sea or sea-coast, and that found in the bowels or among the dung of the whale, either with regard to its qualities and chemical principles, or with respect to the heterogeneous substances that are mixed with it? And 2dly, If there is any such difference, in what does it consist?

From the most exact information I have been able to procure on this subject, I find that what several authors have asserted, that all ambergrise found in whales is of an inferior quality, and therefore much less in price, is destitute of truth. Ambergrise is only valued for its purity, lightness, compactness, colour, and smell. There are pieces of ambergrise found on different coasts, which are of a very inferior quality, whereas there are often found pieces of it in whales of the first value; nay, several pieces found in the same whale, according to the above-mentioned qualities, are more or less valuable. All ambergrise found in whales has at first when taken out of the intestines very near the same smell as the liquid excrements of that animal have; it has then also nearly the same blackish colour: they find it in the whale sometimes quite hard, sometimes rather softish, but never so liquid as the natural faces of that animal. And it is a matter of fact, that, after being taken out and kept in the air, all ambergrise grows not only harder and whiter, but also loses by degrees its smell, and assumes such an agreeable one, as that in general has which is found

swimming



swimming upon the sea; therefore the goodness of ambergrise seems rather to depend on its age. By being accumulated after a certain length of time in the intestinal canal, it seems even then to become of a whiter colour, and less ponderous, and acquire its agreeable smell. The only reason why ambergrise found floating on the sea generally possesses the above-mentioned qualities in a superior degree, is because it is commonly older, and has been longer exposed to the air. It is more frequently found in males than females; the pieces found in females are in general smaller, and those found in males seem constantly to be larger and of a better quality, and therefore the high price in proportion to the size is not merely imaginary for the rarity-sake, but in some respect well founded, because such large pieces appear to be of a greater age, and possess the above-mentioned qualities in general in a higher degree of perfection than smaller pieces.

Having discovered, as I just now mentioned, beaks of the cuttle fish in all the pieces of ambergrise I had an opportunity of examining, it now remained to be ascertained, how those beaks became so constantly mixed with ambergrise? In prosecuting this enquiry, I had the satisfaction to learn from the same persons who gave me the information above-mentioned, that the *Sepia Octopodia*, or cuttle fish, is the constant and natural food of the spermaceti-whale, or *Phyfeter Macrocephalus*. Of this they are so well persuaded, that whenever they discover any recent relics of it swimming on the sea, they conclude that a whale of this kind is, or has been, in that part. Another circumstance which corroborates this fact is, that the spermaceti-whale on being hooked generally vomits up some remains of the *Sepia* \*.

From

\* It will not be improper here to remark, to what an enormous size this species of *Sepia* grows in the ocean. One of the gentlemen who was so kind as to communicate

From what I have said, we may easily account for the many beaks, or pieces of beaks, of the Sepia found in all ambergrise.

The beak of the Sepia is a black horny substance, and therefore passes undigested through the stomach into the intestinal canal, where it is mixed with the fæces; after which it is either evacuated with them, or if these latter be preternaturally retained, forms concretions with them, which render the animal sick and torpid, and produce an obstipation, which ends either in an abscess of the abdomen, as has been frequently observed, or becomes fatal to the animal; whence in both the cases, on the bursting of its belly, that hardened substance, known under the name of ambergrise, is found swimming on the sea, or thrown upon the coast.

From the preceding account, and my having constantly found the above-mentioned beaks of the Sepia in all pieces of ambergrise of any considerable size, I think we may venture to conclude, that all ambergrise is generated in the bowels of the Physeter Macrocephalus, or spermaceti-whale, and there mixed with the beaks of the Sepia Octopodia, which is the principal food of that whale; and we may therefore define ambergrise to be the preternaturally hardened dung or fæces of the Physeter

communicate to me his observations on this subject, about ten years ago hooked a spermaceti-whale that had in its mouth a large substance with which he was unacquainted, but which proved to be a dentaculum of the Sepia Octopodia, nearly 27 feet long: this dentaculum however did not seem to be entire, one end of it appearing in some measure corroded by digestion, so that in its natural state it may have been a great deal longer. With regard to its being a dentaculum of the cuttle fish, the fishermen could not have been mistaken, as they themselves often feed upon the smaller sort of the same Sepia. When we consider the enormous bulk of the dentaculum of the Sepia here spoken of, we shall cease to wonder at the common saying of the fishermen, that the cuttle-fish is the largest fish of the ocean.

Macrocephalus,

Macrocephalus, mixed with some indigestible relics of its food.

There now remains only one objection to be obviated on this subject, and this relates to the chemical analysis of ambergrise\*.

NEUMANN obtained from one drachm of ambergrise five grains of an acid phlegm, two scruples and an half of empyreumatic oil, and two grains of a volatile acid salt in a crystalline form.

Now if all ambergrise owes its origin to the animal kingdom in the manner we have stated, how are we to account for the acid obtained from it by distillation? Would not ambergrise, if it was really of an animal nature, like all other faces of animals feeding upon animal food, yield a volatile alkali? I confess this seems to be a material objection; but I reply to it, first, that although my experiments made upon unadulterated ambergrise confirm those made by NEUMANN, GRIM, BROWNE, and GEORFROY; yet from that analysis it does, in my opinion, by no means follow, that ambergrise is not an animal product.

Two eminent chemists, Mr. SCHEELE, and my friend Mr. BEROMAN, professor of chemistry at Upsal, have lately discovered that human calculi of the bladder, though of an animal origin, are nothing else but a peculiar concrete acid, approaching in its qualities very nearly to the native vegetable acid: and Professor CRELL has lately shewn, in a paper presented to the Royal Society, that the presence of an acid, far from proving any thing against an animal substance, is to be found in the fat

\* Chemistry shews that in all animal excrements an acid is present, though different from that found in ambergrise. Besides, we do not know whether the marine acid of the sea-water in which these animals constantly live, has not a share in changing the nature of their faces; nor whether the faces of all cetaceous animals are perhaps by their chemical analysis not materially different from those of animals living on the Continent. We have a chemical analysis of these latter, but none has been hitherto made of the former,

of

of all animals. This indeed proves as little as if I should conclude on the opposite side of the question, that because the cruciform plants yield first a volatile alkali in distillation, they are of an animal nature. This, however, I have by repeated experiments with *Cochlaria*, *Nasturtium*, &c. seen to be constantly the case. With regard to the nature of the acid which is obtained by distillation from ambergrise, nobody has hitherto to my knowledge examined it; and the experiments I made upon it are insufficient to say any thing positive about it.

The great price of ambergrise (an ounce of it being now sold in London for one pound sterling) has been hitherto the cause of its being so often adulterated, and of its being so little examined by chemists. If, however, a chemical analysis of its acid should be made, we ought to be certain that the ambergrise employed has not been previously adulterated, especially as it is but too common to find it adulterated with flower of rice, or with styrax or other resins, which might deceive us in forming a solid judgement about the real nature of its acid. The adulteration of ambergrise with any of the heterogenous substances may be discovered by its not having all the qualities mentioned above as requisite for the purest and best ambergrise.

The use of ambergrise in Europe is now nearly confined to perfumery, though it has formerly been recommended in physic by several eminent physicians; hence the *Essentia Ambraë Hoffmanni*, *Tinctura Regia Cod. Parisini*, *Trochisci de Ambra Ph. Wurtemberg*, &c. &c.

If we wish to see any medicinal effects from this substance, we must certainly not expect them from two or three grains, but give rather as many scruples of it for a dose; though even then I should not expect much effect from it, as I have taken of pure unadulterated ambergrise in powder 30 grains at once, without

without observing the least sensible effect from it. A sailor, however, who had the curiosity to try the effect of recent ambergrise upon himself, took half an ounce of it melted upon the fire, and found it a good purgative; which proves, that it is not quite an inert substance.

In Asia and part of Africa ambergrise is not only used as a medicine and as a perfume, but a great use is also made of it in cookery, by adding it to several dishes as a spice; a great quantity of it is also constantly bought by the Pilgrims who travel to Mecca, probably to offer it there, and make use of it in fumigations, in the same manner as frankincense is used in Catholic countries. The Turks make use of it as an aphrodisiac. Our perfumers add it to scented pillars, candles, balls or bottles, gloves, and hair-powder; and its essence is mixed with pomatums for the face and hands, either alone or mixed with musk, &c. though its smell is to some persons extremely offensive.

Having now finished my remarks about ambergrise, I shall conclude this paper with some new observations concerning the sebaceous substance generally called Spermaceti, and the whale from which it is obtained.

I mentioned above that it is only one kind of whale from which our fishermen obtain the spermaceti, which they call for this reason the Spermaceti Whale: in this same fish it is that they find ambergrise. They never search after the *Physeter Catodon*, the *Physeter Microps*, *Physeter Turfio*, and others of the same genus; but they aim at taking both the male and female of the *Physeter Macrocephalus*, though the male contains not only a larger quantity, but also in their opinion a better quality of spermaceti.

1st. It is to be observed, that this species has but one spout (*fistula*). This spout is not, as hath been generally hitherto asserted, in the neck (*cervix*) of the fish, but in its front, and on the very edge of the head, bending obliquely on the left side, so that whenever he spouts it is always on that side only.

2dly. It is also remarkable, that the female of this whale has a power of drawing back its breasts after it has suckled the calf, so that it hardly appears to have any prominence on the belly, whereas when it suckles they hang out very long.

3dly. It is not true, though it has hitherto been asserted, that the substance which we so absurdly name *Spermaceti*, and which perhaps might with much greater propriety be called *Sevum Physeteris*, is found in the ventricles of the brain, and in the cavity of the spinal marrow of the *Physeter Macrocephalus*. This fat substance, which is nothing but a kind of suet, undoubtedly formed for some particular purpose of that whale, is contained in a peculiar bony triangular cavity or trunk, which is lodged near the brain, and occupies nearly the whole upper part of the head. This trunk has no communication with the brain, but is entirely separated from it by its bony laminae. The brain, as in all other fishes, is very small in comparison with the size of the whale, and lies directly behind the eyes.

In order to know whether the trunk in which the *spermaceti* is lodged had any connection with the brain of the whale, one of the above-mentioned gentlemen had the curiosity to lance that trunk, which in its upper part is only covered with the skin, he found the whale not in the least affected by this, but on the brain being lanced, the same whale died immediately.



XVI. *Extract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1782. By Thomas Barker, Esquire.*

Read March 13, 1783.

		Barometer.			Thermometer.						Rain.		
					In the House.			Abroad.			Lyndon	Sel- bourn, Hamp.	South Lamb. Surry.
		High.	Low.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.			
Jan.	Morn. Aftern.	30,00	28,45	29,27	49 50	37 38	42½ 43	49 52	21½ 30½	37½ 41½	2,333	4,64	2,23
Feb.	Morn. Aftern.	29,99	28,72	29,50	48 49	33 34	37½ 38½	48 53	23 30½	32 38	0,636	1,98	0,56
Mar.	Morn. Aftern.	29,78	28,57	29,24	47 49	37 38	41 42	47½ 54	23½ 37	34 44	1,923	6,54	2,49
Apr.	Morn. Aftern.	29,69	28,09	29,20	49½ 50	41 42	44 45	45 55	30½ 38½	38 46	6,125	4,57	2,14
May	Morn. Aftern.	29,62	28,54	29,24	58 60	42 43	49 51	58 73½	33 42	44½ 55	5,722	6,34	4,10
June	Morn. Aftern.	30,05	29,06	29,60	68½ 71	50 52	60 61½	67½ 82	44 52	55 66	1,295	1,75	0,49
July	Morn. Aftern.	29,82	29,10	29,51	68 69	57½ 59	61½ 62½	62 75	50½ 55	56 66½	2,697	7,09	6,88
Aug.	Morn. Aftern.	29,64	28,60	29,21	62 63	55 56½	59 60	57½ 70½	46 55	52½ 63½	3,114	8,28	} 7,80
Sept.	Morn. Aftern.	29,89	28,73	29,47	62½ 64	52 53½	58 59½	58 67½	41 50½	51 61½	5,151	3,72	
Oct.	Morn. Aftern.	29,86	28,80	29,40	52½ 54	44½ 45	48½ 49½	52 59½	30 42½	41 50	1,502	1,93	
Nov.	Morn. Aftern.	30,10	28,51	29,40	45 45	34½ 35	39½ 40	42½ 49	23 33	32 38½	1,074	2,51	1,24
Dec.	Morn. Aftern.	30,02	28,95	29,57	44½ 44½	34½ 34½	39 39	43½ 48½	25 30	34 38	0,517	0,91	0,72
32,089											50,26	28,65	

The

The beginning of January was chiefly mild, and some thunder; but as the year advanced it grew more frosty, intermixed with storms and rain, and was a severe latter end of the winter; not long nor settled frosts, but frequent, especially in March, which was almost all either frost and snow, or storms and wet, and was followed by so wet, cold, and backward a season for two months as none remembered. Near twelve inches of rain in April and May, and every thing was six weeks or two months later than usual, and the north and east winds were wet, a sure sign of a wet season; and sometimes there came great rains from the east for two or three days together, and vast floods.

The wall fruit was not only blasted in the blossom, but most of what seemed set fell off afterward, and the leaves and shoots were so much killed, that the trees looked almost dead. The barley seed-time was very bad; a great deal could hardly be sowed at all, or so late it was never well ripened. It appears to have been a very bad season in other parts of Europe also. More frost, and greater snows and rains than usual, in the latter part of winter and spring, even in the southern parts. In May and June there was an almost universal cold or other illness all over Europe, but few entirely escaped it; to many it was but slight, yet in some places it was mortal.

June was the best month this summer, the showers being then fewer and smaller; yet there was never any long continuance of fine weather, but it was soon interrupted either by general wet fits, or by violent and great partial rains and thunder, in particular places, while it was fair elsewhere. In so wet a season hay was, as it might be expected, plentiful, but a great deal of it ill got, and vast quantities of grass the latter part of the summer.



The harvest was very late and tedious. The last week in August, which was the beginning of harvest, was pretty well, and the beginning of September very fine, only the mornings were misty. In this time a good deal of the white corn was well got; but a great part of the barley was not then ripe, and the rest of the harvest, which in some places was not finished till after October 20, was so wet, it was well it could be got in at all tolerably; yet from the coolness of the season, and the unripeness of the barley, very little of it grew in this country or most others. The wheat was but a small crop; the barley almost universally bad. The best crop was that of beans; but hardly any thing was well ripened, and all sorts very dear, wheat three pounds a quarter, barley two pounds, oats one pound.

The three former years were pleasant and fine, chiefly dry, and often hot, but by no means healthy. This, which seemed a very bad one for cold and wet, yet appears to have been more wholesome. The latter part of the summer and the autumn, there has been much less illness about the country than for several years past.

A fortnight in the middle of October was tolerably fair for finishing the harvest; then some storms and rains. The end of October and beginning of November concluded the eight months wet season, for the remainder of the year was dry. Almost all November was much inclined to frost, and sometimes severe, so as to threaten a hard winter. There was a smart frost the latter end of the month, which continued, though with some breaks, above three weeks in November and December before it was quite gone; after which the last fortnight in the year was in general fine, calm, and mild: the  
ground

ground got quite dry in many places, and the wheat, which has lain a great while in the ground, comes up well at last.

*An account of an uncommon circle seen about the Moon.*

November 17, 1782, between ten and eleven at night, there was a remarkable corona about the moon, such as I do not remember to have taken notice of before, at least not to that degree.

It is common, when the moon appears through a thin cloud, to see a bright place round it, bounded by a yellowish red circle at a little distance, which seems to me to be not always of the same diameter. At this time the clear part of the sky was very clear; but there were many thin clouds, and as they passed over the moon from the north, that usual circle appeared much stronger than common, and I should think of less diameter: but the remarkable part was, that round that circle, another rainbow-coloured one was seen; the blue, I think, was on the outside, and the red terminated with the usual red circle. The colours were far more distinct and bright than any halo, and not a third part of that diameter. It was brighter or fainter, according as different parts of the clouds past over the moon; and when the clear sky came over it, the corona very nearly, if not wholly, disappeared.

I have since seen some small resemblance of the same thing, but so faint I should hardly have taken notice of it, if I had not seen it so much stronger at that time.

the 1990s, the number of people in the world who are under 15 years of age is expected to increase from 1.1 billion to 1.5 billion. The number of people aged 65 and over is expected to increase from 200 million to 400 million. The number of people aged 15 and over is expected to increase from 3.5 billion to 4.5 billion. The number of people aged 15 and over is expected to increase from 3.5 billion to 4.5 billion. The number of people aged 15 and over is expected to increase from 3.5 billion to 4.5 billion.

PHILOSOPHICAL  
TRANSACTIONS,  
OF THE  
ROYAL SOCIETY  
OF  
L O N D O N.

V O L. LXXIII. For the Year 1783.

P A R T II.

L O N D O N,

SOLD BY LOCKYER DAVIS, AND PETER ELMSELY,  
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PHILOSOPHICAL  
TRANSACTIONS.

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XVII. *On the proper Motion of the Sun and Solar System; with an Account of several Changes that have happened among the fixed Stars since the Time of Mr. Flamsteed.* By William Herschel, Esq. F. R. S.

Read March 6, 1783.

THE new lights that modern observations have thrown upon several interesting parts of astronomy begin to lead us now to a subject that cannot but claim the serious attention of every one who wishes to cultivate this noble science. That several of the fixed stars have a proper motion is now already so well confirmed, that it will admit of no further doubt. From the time this was first suspected by Dr. HALLEY we have had continued observations that shew Arcturus, Sirius,

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Aldebaran,



Aldebaran, Procyon, Castor, Rigel, Altair, and many more, to be actually in motion; and considering the shortness of the time we have had observations accurate enough for the purpose, we may rather wonder that we have already been able to find the motions of so many, than that we have not discovered the like alterations in all the rest. Besides, we are well prepared to find numbers of them apparently at rest, as, on account of their immense distance, a change of place cannot be expected to become visible to us till after many ages of careful attention and close observation, though every one of them should have a motion of the same importance with Arcturus. This consideration alone would lead us strongly to suspect, that there is not, in strictness of speaking, one *fixed* star in the heavens; but many other reasons, which I shall presently adduce, will render this so obvious, that there can hardly remain a doubt of the general motion of all the starry systems, and consequently of the solar one among the rest.

I might begin with principles drawn from the theory of attraction, which evidently oppose every idea of absolute rest in any one of the stars, when once it is known that some of them are in motion: for the change that must arise by such motion, in the value of a power which acts inversely as the squares of the distances, must be felt in all the neighbouring stars; and if these be influenced by the motion of the former, they will again affect those that are next to them, and so on till all are in motion. Now as we know several stars, in divers parts of the heavens, do actually change their place, it will follow, that the motion of our solar system is not a mere hypothesis; and what will give additional weight to this consideration is, that we have the greatest reason to suppose most of those very stars, which have been observed to move, to be  
such

such as are nearest to us; and, therefore, their influence on our situation would alone prove a powerful argument in favour of the proper motion of the sun, had it actually been originally at rest. But I shall wave every view of this subject which is not chiefly derived from experience.

To begin with my own, I will give a short but general account of the most striking changes I have found to have happened in the heavens since FLAMSTEAD's time. I have now almost finished my third review. The first was made with a Newtonian telescope, something less than 7 feet focal length, a power of 222, and an aperture of  $4\frac{1}{2}$  inches. It extended only to the stars of the first, second, third, and fourth magnitudes. Of my second review I have already given some account\*: it was made with an instrument much superior to the former, of 85.2 inches focus, 6.2 inches aperture, and power 227. It extended to all the stars in HARRIS's maps, and the telescopic ones near them, as far as the eighth magnitude. The catalogue of double stars, which I have had the honour of communicating to the Royal Society, and the discovery of the Georgium Sidus, were the result of that review. My third review was with the same instrument and aperture, but with a very distinct power of 460, which I had already experienced to be much superior to 227, in detecting excessively small stars, and such as are very close to large ones. At the same time I had ready at hand smaller powers to be used occasionally after any particularity had been observed with the higher powers, in order to see the different effects of the several degrees of magnifying such objects. I had also 18 higher magnifiers, which gave me a gradual variety of powers from 460 to upwards of 6000, in order to pursue particular objects to the full extent of

\* Phil. Transf. vol. LXX. LXXI. LXXII.

my telescope, whenever a favourable interval of remarkably fine weather presented me with a proper opportunity for making use of them. This review extended to all the stars in FLAMSTEAD's catalogue, together with every small star about them, as far as the tenth, eleventh, or twelfth magnitudes, and occasionally much farther, to the amount of a great many thousands of stars. To shew the practicability of what I have here advanced, it may be proper to mention, that the convenient apparatus of my telescope is such, that I have many a night, in the course of eleven or twelve hours of observation, carefully and singly examined not less than 400 celestial objects, besides taking measures of angles and positions of some of them with proper micrometers, and sometimes viewing a particular star for half an hour together, with all the various powers of my telescope. The particularities I attended to in this last review were, 1. The existence of the star itself, such as it is given in the British catalogue. 2. To observe well whether it was double or single, well defined or hazy. 3. To view and mark down its particular colour, whenever the altitude and situation of the star would permit it to be done with certainty. 4. To examine all the small stars in the neighbourhood, as far at least as the twelfth magnitude, and note the same particulars concerning them, except the colours, which would have taken up too much time in committing to paper, and be of no very material use. The result of these observations I shall collect under a few general heads in the following articles.

### I.

Stars that are lost, or have undergone some capital change, since FLAMSTEAD's time.

In the British catalogue we find two remarkable stars of the fourth magnitude in the constellation of Hercules, *viz.* the  
80th

80th and 81st. They are no more to be seen. I looked for them in October, 1781, but could not find them; and have since frequently examined that part of the heavens with no better success, though the small stars  $\alpha$ ,  $\zeta$ ,  $\gamma$ , of the sixth magnitude, not far from the place where the former should be, appear very plainly in a fine evening. On referring to my preceding review in August, 1780, I find, that I then also examined the left foot of Hercules, and had these stars been there at that time I must have seen them; for not only  $\alpha$  of the fourth magnitude, but  $\gamma$  of the sixth, is in the list of stars examined; and the place of the 80th and 81st is very near directly between them.

In the northern claw of Cancer FLAMSTEAD has placed three stars of the sixth magnitude; they are the 53d, 55th, and 56th of his catalogue. The latter of them is vanished. Its place and magnitude are so well pointed out by the other two stars, that there can remain no doubt of this remarkable change. We find a very small telescopic star near the place where the 56th should be: this may possibly be the remains of that vanishing star; but that may be ascertained by those astronomical gentlemen, who, having fixed instruments, can determine the place of this small star, and compare it with the 56th of Cancer, when it will appear how far their places agree. I missed it first in February, 1782, and have since looked often for it in vain.

The 19th Persei, a star of the 6th magnitude, is either lost, or so considerably removed from its place in FLAMSTEAD's time, that it is no longer to be known. What gives occasion for a suspicion of its having been removed is, that we find no star of the sixth magnitude in the place where this should be; whereas, about a degree following that situation, there is one of that, or the fifth magnitude, not taken notice of by FLAM-

STEAD, who could not overlook so considerable a star if it had been there in his time ; because, being not far from the parallel of  $\tau$  and  $\nu$ , both which he has given us, it must have passed the field of view of his telescope whenever he observed them.

The 108th Piscium, a star of the sixth magnitude, near the head of Aries is lost. The 107th and 109th, both marked as smaller stars in FLAMSTEAD's catalogue, and near the place of the 108th, are easily discovered.

Two stars of the sixth magnitude, the 73d and 74th Cancri, in the southern claw, are either both lost, or at least have undergone such a remarkable change of magnitude, and one of them of place, that it is hardly possible to know them any longer. The alteration must evidently appear when we compare them to the 81st and 82d Cancri ; the former of which, though only marked of the 7th magnitude, far outshines the brightest of those which may be supposed to be the two stars in question.

The 8th Hydræ is lost. There is a star just by, which I take to be the 31st Monocerotis. If this should be the 8th Hydræ, and a small star near the latter should agree with the place of the 31st Monocerotis, then the magnitudes will be quite contrary to what FLAMSTEAD makes them. There must, at all events, have been a very remarkable change.

The 26th Cancri is lost. Near this star is placed the 22d of the same constellation, and, as their distance is not much more than a quarter of a degree, it requires fixed instruments to determine which of the two is the star wanting : from the magnitude, however, I surmise, that the remaining star is the 22d rather than the 26th.

The 62d Orionis is lost ; and a star near the 54th and 51st is not taken notice of by FLAMSTEAD. Perhaps the 62d has changed place ; if this should be the case, it must have a very considerable motion.

The

The 71st Herculis, a star of the 5th magnitude, is lost. The 70th and 71st are so near each other by FLAMSTEAD's catalogue, that it cannot be determined without fixed instruments, which is the star wanting. There is a small telescopic star, within about 30 minutes north following, in a direction towards  $\mu$  Lyræ; if that should be the 71st, it is wonderfully changed both in place and size. The 40th star in Mr. MAYER's collection of double stars \* seems to be the 70th Herculis of FLAMSTEAD. Now, as that star is perfectly single in my telescope, with every power I have tried upon it, we may surmise that one of the stars which is now vanished was still visible in the year 1778, when Mr. MAYER observed it, though then already diminished from the 5th to the 8th magnitude.

The 34th Comæ Berenices is lost: FLAMSTEAD has marked it as a star of the 5th magnitude.

The 19th of the same constellation is also lost, or moved and changed in magnitude.

The 40th and 41st Draconis have undergone so great an alteration of place that we cannot possibly mistake it; for in FLAMSTEAD's time they were above three minutes asunder, whereas now their distance is much less than half a minute. A more particular account of these two stars will be given in a second collection of more than 400 new double stars, observed in my third review, which I hope soon to have the honour of presenting to the Royal Society.

There seems to be an alteration in the place of the 65th, 64th, 54th, and 57th Orionis; but without fixed instruments I cannot ascertain in which of the stars it is. Their situation in the heaven does not agree with that which is delineated in FLAMSTEAD's Atlas Cœlestis, for these two pair of stars are

\* De novis in Cœlo sidereo phænomenis.

much nearer now than they should be, according to that account.

## II.

Stars that have changed their magnitude since FLAMSTEAD'S time.

$\alpha$  Draconis is so much less than  $\beta$ , which is set down as a smaller star in FLAMSTEAD'S catalogue, that the change of magnitude cannot be doubted.

$\beta$  Ceti marked of the 3d, and  $\alpha$  Ceti of the 2d, are evidently the reverse,  $\beta$  being by much the larger star. I have mentioned this circumstance in my observations on the periodical star in Collo Ceti\*, and it seems now as if the difference between the magnitudes of these two stars was still increasing.

$\zeta$  Serpentis is not near so large as  $\eta$ , and yet we find FLAMSTEAD has placed them in the same class: however, we cannot intirely confide in the marks of the magnitudes when two stars are placed in the same class, since every order admits of a considerable variety; but when the marks contradict experience so far as to describe one star, for instance, of the third, and another of the 4th magnitude, when observation shews the latter to be of the 3d and the former of the 4th, I think we can hardly doubt but that there must have been a change.

$\eta$  Cygni is a brighter star than  $\chi$ , though marked by FLAMSTEAD of a less magnitude.

The 2d Ursæ minoris is marked of the 6th magnitude, but is certainly intitled to the 5th.

$\eta$  Bootis is much larger than  $\zeta$ .

$\delta$  Delphini is much larger than  $\kappa$ .

$\beta$  Trianguli is much larger than  $\alpha$ .

$\gamma$  Aquilæ is much larger than  $\beta$ .

\* Phil. Trans. vol. LXX. numb. XXI.

$\sigma$  Sagittarii is larger than  $\delta$ ,  $\gamma$ , and  $\epsilon$ , though marked of an inferior magnitude.

$\delta$  Canis majoris is larger than  $\beta$ , and yet is marked to be less.

$\eta$  Serpentis is so much larger than  $\zeta$ , that they certainly should not have been put in the same order of magnitude.

$\alpha$  Serpentarii is larger than  $\gamma$  and  $\epsilon$ , though marked to be of a less magnitude than either.

$\beta$  Equulei is so much less than  $\alpha$  that it could hardly deserve to be put in the same class.

$\delta$  Delphini is larger than  $\epsilon$ , though placed in an inferior order.

$\epsilon$  Bootis is so much larger than  $\zeta$  that it should not be put into the same order.

$\delta$  Sagittæ is larger than  $\alpha$  and  $\beta$ , though placed in a lower order of magnitude.

$\delta$  Ursæ majoris is less than either  $\epsilon$ ,  $\zeta$ , or  $\eta$ , though it is marked of a superior order of magnitude. Besides, it is evidently visible, that  $\delta$  cannot be intitled to more than the 4th magnitude, or at most to between the 4th and 3d: on the contrary,  $\epsilon$ ,  $\zeta$  and  $\eta$ , should be of the 2d, or at least between the 2d and 3d; all which is very different from FLAMSTEAD'S account of those remarkable stars.

$\alpha$  Ursæ majoris is less than any star marked of the same magnitude, and cannot have the least pretension to be called a star of between the 1st and 2d, as FLAMSTEAD has marked it, and as I make no doubt it was in his time.

The 1st and 2d Hydræ are noted by FLAMSTEAD as being of the 4th magnitude, whereas they now are only of the 8th or 9th. It is remarkable, that the 30th Monocerotis, which is situated between them, has retained the order assigned to it



by FLAMSTEAD, and being of the 6th serves to point out the change of the other two in a very evident manner.

$\gamma$  Lyrae is much larger than  $\beta$ .

The change in the magnitudes of the 31st and 34th Draconis is very striking, these two stars being just the contrary of what they are marked in FLAMSTEAD's catalogue. The 31st from the 7th is increased to the 4th; and the 34th, from being a star between the 4th and 5th, is reduced to one of the 6th, if not 7th magnitude.

The 44th Cancræ is much too small for the 6th magnitude. As  $\epsilon$  and others are marked of the 6th, this, on being compared to them, can be intitled to no more than the 8th or 9th order.

The 96th Tauri is small enough to be of the 8th magnitude, though marked as one of the 6th.

The 62d Arietis is of the 5th magnitude, though only marked of the 6th.

The magnitudes of the 12th and 14th Lyncis are just the reverse in the heavens to what FLAMSTEAD has marked them. This denotes a double change of a star from the 5th to the 7th, and from the 7th to the 5th magnitude.

The 38th Persei, marked of the 6th magnitude, is increased so as to be equal to  $\theta$  and  $\pi$  of the 4th. Also,  $\theta$  is less than  $\tau$  contrary to FLAMSTEAD.

The 8th Monocerotis is less than the 76th Orionis, though the former should be of the 4th, and the latter only of the 6th magnitude.

The 23d Geminorum, though marked of the 5th, is less than the 21st of between the 6th and 7th magnitude.

The

The 26th Orionis is much too small for the magnitude of which it is marked to be, or rather is lost; for I can hardly take any one of the remaining telescopic stars for it.

ξ Leonis in FLAMSTEAD's time was of the 4th; but is now less than a star of the 5th magnitude.

### III.

Stars newly come to be visible.

Near Lacerta's tail-end is a star of between the 4th and 5th magnitude, not mentioned in FLAMSTEAD's catalogue, though the 1st Lacertæ, not far from that place, is recorded. It is so easy to be seen with the naked eye, and in a spot where but few stars of that magnitude are near, that we can hardly account for its being omitted if it had been visible to FLAMSTEAD. Its colour is pale red.

The star of the 5th magnitude following τ Persei, supposed to be removed, is most likely new, unless future observations were to favour the supposed motion of this star. It is among the double stars of my 4th class, so that it will be easy to detect its proper motion.

A very considerable star, not marked by FLAMSTEAD, will be found near the head of Cepheus. Its right ascension in time, is about 2<sup>h</sup> 19<sup>m</sup> preceding FLAMSTEAD's 10th Cephei, and it is about 2° 20' 3" more south than the same star. It is of a very fine deep garnet colour, such as the periodical star α Ceti was formerly, and a most beautiful object, especially if we look for some time at a white star before we turn our telescope to it, such as α Cephei, which is near at hand.

A considerable star in a direction from the 68th Geminorum towards the 61st is not to be found in FLAMSTEAD, its colour is red.

A star of a considerable magnitude preceding the 1st Equulei is not contained in FLAMSTEAD's catalogue. It is a double star of the first class, the 61st of my second collection, where measures of it will be found.

A considerable star following the 1st Sextantis, and another following the 7th, are not inserted.

Between  $\beta$  Cancræ and  $\delta$  Hydræ is a very considerable star not marked by FLAMSTEAD, though its situation is very remarkable. As the constellation of Cancer contains so rich a collection of very small stars, it is to be wondered how a star of such consequence could be omitted, if it had been visible in FLAMSTEAD's time.

Nearly  $1\frac{1}{2}$  degree north following  $\delta$  Herculis, almost in the direction of  $\delta$  and  $\nu$ , is a star of the 5th, or between the 4th and 5th magnitude, very visible to the naked eye. We can hardly think FLAMSTEAD could have overlooked it, had it been there in his time.

About 3 degrees south preceding  $\gamma$  Bootis, a considerable star not in FLAMSTEAD's catalogue of the 6th magnitude; and south preceding  $\lambda$ , another, almost as large.

Here we ought to observe, that it is not easy to prove a star to be newly come; for though it should not be contained in any catalogue whatsoever, yet the argument for its former non-appearance, which is taken from its not having been observed, is only so far to be regarded as it can be made probable, or almost certain, that a star would have been observed had it been visible. For these reasons I will lay no particular stress on the new appearance of the above stars; they are, however, such as do well deserve to have their places settled, while I shall leave it to others to determine how far they may think them to be new visitors to those starry regions that fall within the reach of our sight.

To

To return to the principal subject of this paper, which is the proper motion of the sun and solar system; does it not seem very natural, that so many changes among the stars,—many increasing their magnitude, while numbers seem gradually to vanish;—several of them strongly suspected to be new-comers, while we are sure that others are lost out of our sight;—the distance of many actually changing, while many more are suspected to have a considerable motion:—I say, does it not seem natural that these observations should cause a strong suspicion that most probably every star in the heaven is more or less in motion? And though we have no reason to think, that the disappearance of some stars, or new appearance of others, nor indeed the frequent changes in the magnitudes of so many of them are owing to their change of distance from us by proper motions, which could not occasion these phenomena without being inconceivably quick; yet we may well suppose, that motion is some way or other concerned in producing these effects. A slow motion, for instance, in an orbit round some large opaque body, where the star, which is lost or diminished in magnitude, might undergo occasional occultations, would account for some of those changes, while others might perhaps be owing to the periodical return of large spots on that side of the surface which is alternately turned towards us by a rotatory motion of the star. The idea also of a body much flattened by a quick rotation, and having a motion similar to the moon's orbit by a change of the place of its nodes, whereby more of the luminous surface would one time be exposed to us than another, tends to the same end; for we cannot help thinking with Mr. DE LA LANDE (Mem. 1776), that the same force which gave such rotations, would probably  
also.

also occasion motions of a different kind by a translation of the center \*. Now, if the proper motion of the stars in general be once admitted, who can refuse to allow that our sun, with all its planets and comets, that is, the solar system, is no less liable to such a general agitation as we find to obtain among all the rest of the celestial bodies †.

Admitting this for granted, the greatest difficulty will be how to discern the proper motion of the sun between so many other (and variously compounded) motions of the stars. This is an arduous task indeed, which we must not hope to see accomplished in a little time; but we are not to be discouraged from the attempt. Let us, at all events, endeavour to lay a good foundation for those who are to come after us. I shall therefore now point out the method of detecting the direction and quantity of the supposed proper motion of the sun by a few geometrical deductions, and at the same time shew by an application of them to some known facts, that we have already some reasons to guess which way the solar system is probably tending its course.

Suppose the sun to be at S, fig. 1.; the fixed stars to be dispersed in all possible directions and distances around at  $s, s, s, s,$  &c. Now, setting aside the proper motion of the stars, let us first consider what will be the consequence of a proper motion in the sun; and let it move in a direction from A towards B.

\* Relating to the motion of the fixed stars, the Astronomer Royal has an expression in the second page of the explanation and use of the tables published in his *Astronomical Observations*, which seems to favour this idea, where he mentions the “peculiar but small motions, which many, IF NOT ALL OF THEM, have among themselves, which have been called their *proper motions*, the causes and laws of which are hid for the present in almost equal obscurity.”

† See Mr. MICHELL's note, *Phil. Trans.* vol. LVII. p. 252.

Suppose it now arrived at C. Here, by a mere inspection of the figure, it will be evident, that the stars  $s, s, s$ , which were before seen at  $a, a, a$ , will now, by the motion of the sun from S to C, appear to have gone in a contrary direction, and be seen at  $b, b, b$ ; that is to say, every star will appear more or less to have receded from the point B, in the order of the letters  $ab, ab, ab$ . The converse of this proposition is equally true; for if the stars should all appear to have had a retrograde motion, with respect to the point B, it is plain, on a supposition of their being at rest, the sun must have a direct motion towards the point B, to occasion all these appearances. From a due consideration of what has been said, we may draw the following inferences.

1. The greatest or total systematical parallax of the fixed stars, fig. 2. will fall upon those that are in the line DE, at rectangles to the direction AB of the sun's motion.

2. The partial systematical parallax of every other star,  $s, s, s$ , not in the line DE, will be to the total parallax as the sine of the angle  $BSs$ , being the stars distance from that point towards which the sun moves, to radius.

3. The parallax of stars at different distances will be inversely as those distances; that is, one half at double the distance, one third at three times, and so on; for the subtense SC remaining the same, and the parallactic angle being very small, we may admit the angle  $SsC$ , to be inversely as the side  $Ss$ , which is the stars distance.

4. Every star at rest, to a system in motion, will appear to move in a direction contrary to that in which the system is moving.

Corollary. Hence it follows, that if the solar system be carried towards any star situated in the ecliptic: every star, whose angular

angular distance *in antecedentia* (reckoned upon the ecliptic from the star towards which the system moves) is less than 180 degrees, will decrease in longitude. And that, on the contrary, every star, whose distance from the same star (reckoned upon the ecliptic but *in consequentia*) is less than 180 degrees, will increase in longitude, in both cases without alteration of latitude.

From these principles it would be easy to draw general theorems for every possible direction of the motion of the solar system, by which we might find what alteration of longitude or latitude would take place in any given star; but it will be time enough for those investigations when we shall have more immediate occasion for them. What we are now chiefly to endeavour at is, the speedy method of obtaining sufficient facts to proceed upon.

The immense regions of the fixed stars may be considered as an infinitely expanded globe, having the solar system for its center. With this idea it will occur to us, that no method can be so proper for finding out the direction of the motion of the sun as to divide our observations on the systematical parallax of the fixed stars into three principal zones. These, for the convenience of fixed instruments, may be assumed so as to let them pass around the equator and the equinoctial and solstitial colures, every one being at right angles to the other two, according to the three dimensions of solids. And since no observations can be so conveniently made to ascertain small relative proper motions among the fixed stars as those on double stars, I have continued my researches in that line with great application, and can now furnish out these three zones, with a very complete set of double stars for such observations. We have the greatest reason to hope for success in this attempt; for,

if

if I am not mistaken, there will be found a secular systematical parallax of some considerable value; nay, possibly, so short a space of time as ten years may suffice to bring us acquainted with many hitherto unknown celestial motions.

The equatorial zone, extending 10 degrees on each side of the equator, will contain about 150 stars which I have found to be double, *viz.*

Piscium 38. 51. 77. 86. north of 110. 113.

Ceti south of 13. foll. 25. 26. 37. north of 37. foll. 54. 61. 66.

Eridani 32. near 48. 62. 69.

Tauri near 10. 45. foll. 66. 88.

Orionis foll. 1. near 10. foll. 10. 19. pre. 20. 20. 23. near 26. 28. near 28. pre. 29. near 30. between 30 and 33. 32. 33. 34. foll. 34. pre. 36. 39. 41. near 42. 44. another 44. pre. 46. north of 46. foll. 47. 48. 50. 52. pre. 58. 58. 59.

Monocerotis 8. 11. near 11. In naribus. Sub genam. Inter pedes. near 12. foll. 15. pre. 25. foll. 25. 29. 31. near 31.

Canis minoris near 10. foll. 10. 14.

Canceri 17.

Hydræ pre. 4. pre. 4. foll. 4. 15. 17. pre. 22. 22. foll. 22. 27. 30. 31.

Leonis 3. foll. 3. pre. 43. south of 43. 57. foll. 63. 74. pre. 75. 83. 84.

Virginis 4. 17. 25. 29. pre. 44. 44. 51. 51. 84. pre. 93. 93.

Libræ 17. near 31.

Serpentis 58. 59. 63.

Serpentarii near 11. 53. 61. 67. south of 67. 70.

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Aquilæ



Aquilæ 2. near 6. near 6. pre. 7 and 8. 15. 24. pre. 30  
near 35. near 35. 49. 53. near 54. 57. near 63. 64. near  
65. near 65.

Delphini 1.

Aquarii 4. 4. 22. 24. 51. 55. south of 72.

Equulei 1. pre. 1. south of 2. south of 6. 7.

Pegasi 3. near 3. 8. near 18.

Piscium near 7. 8. south of 10. 35.

Ceti south of 4.

The zone of the equinoctial colure, extending 10 degrees of  
a great circle on each side, will contain, as far as it is visible in  
our hemisphere, about 70 double stars, *viz.*

Ceti foll. 4. near 13.

Aquarii 107.

Piscium 51. 38. 35. 76.

Andromedæ 21. Supra caput. 29. pre. 23. near 27. near  
16. near 17.

Cassiopeæ pre. 25. prec. 25. 8. 24. 18. 6. 9. south of 11.

55. 34. 31. 4. 3. 2. near 33. 35. 36. near 3. 44. 47.

Cephei 1. foll. 32. foll. 32. foll. 31.

Ursæ minoris 1. 18.

Draconis 40 and 41. between 10 and 11. near 77.

Ursæ majoris 79. between 50 and 38. foll. 42. 65. 57. near  
58. south of 69.

Canum venaticorum 12. 2.

Comæ Berenices 24. from 36 to 26. 2. 12.

Leonis 95. 90. 93. pre. 95.

Virginis 29. pre. 44. between 4 and 6. 17. 25. 27. south  
of 12. 4.

Corvi 7.

Crateris 17. foll. 21.

The

The zone of the solstitial colure, of the same extent, will include about 120 double stars, viz.

Canis majoris north of 13. 17. foll. 5. foll. 2. foll. 5.

Monocerotis 11. 8. 12. near 11. In naribus. foll. 15. Inter pedes. Sub genam.

Leporis 13.

Orionis 48. near 42. 41. 44. 44. 50. foll. 72. 58. In fuste. 59. pre. 70 and 76. pre. 58. pre. 69.

Geminorum 43. 38. pre. 1. between 13 and 18. 21. 15. north of 19. 12. near 34. 27. 37. near 24. near 24. south of 18. foll. 13 and 18.

Tauri north of 123.

Aurigæ 29. 13. 37. 34. 32. south of 34. 56. near 59. pre. 58. pre. Nebulam. 41.

Lyncis 12. In naribus. 5. 13. In pectore. 19.

Camelopardali. In aure.

Ursæ minoris 1. near 18. near 12.

Draconis 39. 63. 19. 21. 56. 31. 47. 24 and 25. 69. 46. near 31. 40 and 41. 48. near 23. near 77.

Cephei 1. 8.

Herculis 75. 95. over 85. near 87. 100. 86. near 94. near 103.

Lyræ 4. 5. 6. 10. 3. 8. 11. near 3. near 6 and 7. foll. 5. foll. 10. pre. 10. pre. 14 and 15.

Serpentarii 70. 54. 61. 53. 67. near 67. north of 72. north of 72.

Serpentis 59. 63. 58.

Aquilæ near 7 and 8. 2. near 6. near 6.

Sagittarii 13. 38.

It will not be amiss to add a zone of the ecliptic, which will contain, among others, a great many double stars that may

N n 2

undergo

undergo occultations by the moon or planets. This is of the same extent, and includes about 120 double stars.

Arietis north of 3. 5. pre. 6. north of 6. pre. 17. 30. 42.

pre. 54. foll. 62. pre. 63. foll. 63. foll. 63. foll. 63.

Tauri near 4. 7. pre. 8. foll. 11. 30. 52. 59. 62. 68. foll.

68. pre. 74. 87. 94. 103. north of 103. 105. 111. foll.

112. 114. foll. 117. 118. north of 123.  $\alpha$ .

Orionis 68. pre. 69. pre. 70 and 76.

Aurigæ 14. 26. pre. Nebulam.

Geminorum pre. 1. 4. 12. foll. 13 and 18. 15. 18. 21.

pre. 24. near 24. near 24. 27. 37. 38. 54. foll. 55. pre.

61. 63. north of 63. 66. 78. foll. 78. foll. 81.

Cancrî 11. 16. foll. 16. 22. 23. 24. 30. 48. 54. pre. 77.

$\omega$  foll.  $\iota$ .

Leonis 2 3. foll. 3. 6. 7. 14. 25. 32. 41. south of 43.

pre. 43. foll. 44. 57. foll. 63. 74. pre. 75. 83. 84.

Virginis 4 foll. 4. pre. 4. 17. 25. 44. 51.

Hydræ 54.

Libræ pre. 12. 18. 24. 31. 51.

Scorpii 8. 14.

Serpentarii 5. 38. 39.

Sagittarii 13. 38. foll. 43. 64. near 65.

Capricorni north of 1. 5. 7. 9. 11. 12. near 29. 29. 39.

Aquarii 14. 22. foll. 43. 51. 55. 57. 70. 71. 72. 91. 94.

Piscium near 7. south of 8. 35. 38. 51. 77. 86. 110. 113.

Ceti foll. 4. near 13. 26. 54.

An account of each of these double stars, not already in my first catalogue, will be contained in the second collection. It remains now only for me to make an application of this theory to some of the facts we are already acquainted with, relating to the proper motion of the stars. And first let me observe, that  
the

the rules of philosophizing direct us to refer all phenomena to as few and simple principles as are sufficient to explain them. Thus, for instance, we see the stars and planets rise and set every day: now, as it is much more simple to admit the earth to turn once in 24 hours, than to suppose every single star to revolve round the earth in that time, we very justly ascribe a diurnal motion to the earth; but yet, since we find that the planets do not every night exactly retain their relative places among the stars, we next admit that such deviations from the law, which all the rest seem to obey, are owing to a proper motion of their own. To apply this to the solar system.—Astronomers have already observed what they call a proper motion in several of the fixed stars, and the same may be supposed of them all. We ought, therefore, to resolve that which is common to all the stars, which are found to have what has been called a proper motion, into a single real motion of the solar system, as far as that will answer the known facts, and only to attribute to the proper motion of each particular star the deviations from the general law the stars seem to follow in those movements.

By Dr. MASKELYNE's account of the proper motion of some principal stars\*, we find that Sirius, Castor, Procyon, Pollux, Regulus, Arcturus, and  $\alpha$  Aquilæ, appear to have respectively the following proper motions in right ascension.  $-0''.63$ ;  $-0''.28$ ;  $-0''.80$ ;  $-0''.93$ ;  $-0''.41$ ;  $-1''.40$ ; and  $+0''.57$ ; and two of them, Sirius and Arcturus, in declination, viz.  $1'.20$  and  $2''.01$ , both southward. Let fig. 3. represent an equatorial zone, with the above mentioned stars referred to it, according to their respective right ascensions,

\* Astronomical Observations made at the Royal Observatory at Greenwich.

having the solar system in its center. Assume the direction AB from a point somewhere not far from the 77th degree of right ascension to its opposite 257th degree, and suppose the sun to move in that direction from S towards B; then will that one motion answer that of all the stars together: for if the supposition be true, Arcturus, Regulus, Pollux, Procyon, Castor, and Sirius, should appear to decrease in right ascension, while  $\alpha$  Aquilæ, on the contrary, should appear to increase. Moreover, suppose the sun to ascend at the same time in the same direction towards some point in the northern hemisphere, for instance, towards the constellation of Hercules; then will also the observed change of declination of Sirius and Arcturus be resolved into the single motion of the solar system. I am well aware of the many yet remaining difficulties, such as the correspondence of the exact quantity of each star's observed proper motion with the quantity that will be assigned to it by this hypothesis; but we ought to remember, that the very different and still unknown relative distances of the fixed stars must, for a good while yet, leave us in the dark about the particular and strict application of the theory; and that any deviation from it may easily be accounted for by the still unknown *real* proper motion of the stars: for if the solar system have the motion I ascribe to it, then what astronomers have already observed concerning the change of place of the stars, and have called their proper motion, will become only an *apparent* motion; and it will still be left to future observations to point out, by the deviations from the general law which the stars will follow in those apparent motions, what may be their real proper motions as well as relative distances. But lest I should be censured for admitting so new and capital a motion upon too slight a foundation,

dation, I must observe, that the concurrence of those seven principal stars cannot but give some value to an hypothesis that will simplify the celestial motions in general. We know that the sun, at the distance of a fixed star, would appear like one of them; and from analogy we conclude the stars to be suns. Now, since the apparent motions of these seven stars may be accounted for, either by supposing them to move just in the manner they appear to do, or else by supposing the sun alone to have a motion in a direction, somehow not far from that which I have assigned to it, I think we are no more authorised to suppose the sun at rest than we should be to deny the diurnal motion of the earth, except in this respect, that the proofs of the latter are very numerous, whereas the former rests only on a few though capital testimonies. But to proceed: I have only mentioned the motions of those seven principal stars, as being the most noticed and best ascertained of all; I will now adduce a farther confirmation of the same from other stars.

M. DE LA LANDE gives us the following table of the proper motion of 12 stars, both in right ascension and declination, in 50 years\*.

\* *Ast. par M. DE LA LANDE, tom. IV. p. 685.*

Etoiles.	Chang. d'asc. droite.	Chang. de déclinaison.
Arcturus	- 1 11	- 1 55
Sirius	- 37	- 52
$\beta$ Cygni	- 3	+ 49
Procyon	- 33	- 47
$\epsilon$ Cygni	+ 20	+ 34
$\gamma$ Arietis	- 14	- 29
$\gamma$ Gemin.	- 8	- 24
Aldébaran	+ 3	- 18
$\beta$ Gemin.	- 48	- 16
$\gamma$ Piscium	+ 53	+ 7
$\alpha$ Aquilæ	+ 32	- 4
$\alpha$ Gemin.	- 24	- 1

Fig. 4. represents them projected on the plane of the equator. They are all in the northern hemisphere, except Sirius, which must be supposed to be viewed in the concave part of the opposite half of the globe, while the rest are drawn on the convex surface. Regulus being added to that number, and Castor being double, we have 14 stars. Every star's motion, except Regulus, is assigned in declination as well as in right ascension, so that we have no less than 27 motions given to account for. Now, by assuming a point somewhere near  $\lambda$  Herculis, and supposing the sun to have a proper motion towards that part of the heaven, we shall satisfy 22 of these motions. For  $\beta$  Cygni,  $\alpha$  Aquilæ,  $\epsilon$  Cygni,  $\gamma$  Piscium,  $\gamma$  Arietis, and Aldebaran, ought, upon the supposed motion of the sun, to have an apparent progression, according to the hour circle XVIII, XIX, XX, &c. or to increase in right ascension, while Arcturus, Regulus, the two stars of  $\alpha$  Geminorum, Pollux, Procyon, Sirius, and  $\gamma$  Geminorum, should apparently go

go back in the order XVI, XV, XIV, &c. of the hour circle, so as to decrease in right ascension; but according to M. DE LA LANDE's table, excepting  $\beta$  Cygni and  $\gamma$  Arietis, all these motions really take place. With regard to the change of declination, we see that every star in the table should go towards the south; and here we find but three exceptions in  $\beta$  and  $\epsilon$  Cygni, and  $\gamma$  Piscium; so that upon the whole we have but five deviations out of 27 known motions which this hypothesis will not account for. And these exceptions must be resolved into the real proper motion of the stars.

There are also some very striking circumstances in the quantities of these motions that deserve our notice. First, Arcturus and Sirius being the largest of the stars, and therefore probably the nearest, ought to have the most apparent motion, both in right ascension and declination, which is agreeable to observation, as we find by the table. Next, in regard to the right ascension only, Arcturus being better situated to shew its motion, by theorem II. p. 261. ought to have it much larger, which we find it has. Aldebaran, both badly situated and considerably smaller than the two former, by the same theorem ought to shew but little motion. Procyon, better situated than Sirius, though not quite so large, should have almost as much motion; for by the third theorem, on supposing it farther off because it appears smaller, the effect of the sun's motion will be lessened upon it; whereas, on the other hand, by the second theorem, its better situation will partly compensate for its greater distance. This again is conformable to the table.  $\epsilon$  Cygni very favourably situated, though but a small star, should shew it considerably as well as  $\alpha$  Aquilæ; whereas  $\beta$  Cygni should have but little motion: and  $\gamma$  Piscium, best



situated of all, should have a great increase of right ascension, and these deductions also agree with the table.

In the last place, a very striking agreement with the hypothesis is displayed in Castor and Pollux. They are both pretty well situated, and we accordingly find that Pollux, for the size of the star, shews as much motion in right ascension as we could expect; but it is remarkable, and seemingly contrary to our hypothesis, that Castor, equally well placed, shews by the table no more than one half of the motion of Pollux. Now, if we recollect that the former is a double star, consisting of two stars not much different in size, we can allow but about half the light to each of them, which affords a strong presumption of their being at a greater distance, and therefore their partial systematical parallax, by the third theorem, ought to be so much less than that of Pollux, which agrees wonderfully with observation\*. Not to mention the great difficulty in which we should be involved, were we to suppose the motion of Castor to be really in the star: for how extraordinary must appear the concurrence, that two stars, namely those that make up this apparently single star, should both have a proper motion so exactly alike, that in all our observations hitherto they have not been found to disagree a single second, either in right ascension or declination, for fifty years together! Does not this seem strongly to point out the common cause, the motion of the solar system?

\* If the light of Castor was exactly equal to that of Pollux, and the two stars, which make up the former star, were perfectly of the same size, we might, on that account, suppose the distance of Castor from us to be to that of Pollux as  $\sqrt{2} : 1$ ; but Castor is in fact something less bright; and this consideration, added to the former, will make it probable enough that its distance may perhaps be double that of Pollux.

With

With respect to the change of declination I would observe, that the point of  $\lambda$  Herculis, which in fig. 4. is assumed as the Apex \* of the solar motion is not perhaps the best selected. A somewhat more northern situation may agree better with the changes of declination of Arcturus and Sirius, which capital stars may perhaps be the most proper to lead us in this hypothesis; but as we should be guided by facts in researches of this nature, it may be as well to expect the assistance of future observations before we are too particular in determining this point †.

It may be expected I should also mention something concerning the quantity of the solar motion; but here I can only offer a few distant hints. From the annual parallax of the fixed stars, which, from my own observations, I find much less than it has hitherto been proved to be, we may certainly admit (without entering into a subject which I reserve for a future opportunity) that the diameter of the earth's orbit, at the distance of Sirius or Arcturus, would not nearly subtend an angle of one second; but the apparent motion of Arcturus, if owing to a translation of the solar system, amounts to no less than  $2''.7$  a year; as will appear if we compound the two motions of  $1' 11''$  in right ascension, and  $1' 55''$  in declination, into one single motion, and reduce it to an annual quantity.

\* I use the term Apex here to denote that point of fig. 4. wherein all great circles, drawn through the supposed direction of the motion of the solar system, intersect, and which, in other stereographic projections, is generally a pole, either of the ecliptic or equator. As this point is in the northern or elevated hemisphere, the sun, by tending to it, may be said to ascend, and the term Apex may perhaps not be an improper one.

† From the additional testimony of other capital stars considered in the postscript it now appears, that the point of  $\lambda$  Herculis is probably as well chosen as any we can fix upon in that part of the heavens.

Hence we may in a general way estimate, that the solar motion can certainly not be less than that which the earth has in her annual orbit.

I have now only to add, that it is to be expected future observations will soon throw more light upon this interesting subject, and either fully establish or overturn the hypothesis of the motion of the whole solar system. To this end I have already begun a series of observation upon several zones of double stars; and should the result of them be against these conjectures, I shall be the first to endeavour to point out the fallacy of them.

Datchet near Windsor,

Feb. 1, 1783.

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*Postscript to the Paper on the Motion of the Solar System.*

In my paper on the Motion of the Solar System, I used a table of the proper motion of some fixed stars, which M. DE LALANDE has given us as an extract from TOB. MAYER's Opera inedita. By the favour of my astronomical friend Mr. AUBERT, I am now furnished with the scarce edition of the original. This work contains a catalogue of the place of 80 stars, observed by Mr. MAYER in 1756, and compared with the same stars as given by ROEMER in 1706. From the goodness of the instrument with which the observations to which Mr. MAYER has compared his own were made, he gives it as his opinion, that where the disagreement in the place of a star is but small, it may

may be attributed to the imperfection of the instrument; but that when it amounts to 10 or 15'', it is a very probable indication of a proper motion of such a star. He adds, that when the disagreement is so much as in some stars which he names, (among which is FOMAHAND, where the difference is 21'' in 50 years) he has not the least doubt of a proper motion\*.

By this extensive table I thought it highly necessary immediately to examine the hypothesis of the motion of the solar system, that it might receive an early check from observations, if they should be unfavourable; or that, on the other hand, it might be supported by the additional evidence of more stars, if their apparent proper motions should coincide with the idea I have pointed out in my paper on this subject.

I have followed Mr. MAYER's judgement of his own and ROEMER's observations, and left out of the list all the stars that do not shew a disagreement amounting to 10'' in the places which are given for them in 1706 and 1756. I have also left out those 13, or rather 14 stars, which have already been examined in my paper, and have been shewn to support the hypothesis I have advanced: the rest are here drawn up in two tables. The first contains the stars that agree with my assigned motion of the solar system; or rather which are thereby resolved into apparent, or partly apparent, and partly proper motions. The second table contains those stars whose motions cannot be accounted for by my hypothesis, and must therefore be ascribed to a real motion in the stars themselves, or to some *still more hidden* cause of a *still remoter* parallax†.

\* De motu fixarum proprio Commentatio. Op. ined. Vol. I. p. 79.

† That I may not be obscure, it will be proper to mention what I allude to, especially as it claims a distant connection with our subject, and may hereafter become of sufficient moment to engage our attention. Mr. MICHELL's admirable  
idea.

idea of the stars being collected into systems (Phil. Transf. vol. LI, p. 249.) appears to be extremely well-founded, and is every day more confirmed by observations: though this does not, in my opinion, take away the probability of many stars being still as it were *solitary*, or, if I may use the expression, *inter systematical*. It occurs then naturally, that by the principle of gravitation, which is never at rest, and which we have no reason to doubt extends to all possible distances, one system of stars will act on another as if the stars of each system were all collected into the center of gravity of each respective system. Hence then will arise this evident consequence, that a star, or sun, such as ours, may have a proper motion within its own system of stars, while at the same time the whole stary system to which it belongs may have another proper motion, totally different in quantity and direction. It will require no little abstract consideration to conceive the possibility of what may be thus surmised; therefore an instance or two, to elucidate the matter, may not be improper. If an inhabitant of the 5th satellite of Saturn should have discovered, that his little world revolves at a great distance round a planet, and to his great astonishment should also have found, that this planet again revolves round the sun;—if, farther, our hypothesis of the solar motion should prove to be well-founded (which, in some of the stars, supposing them to be suns surrounded with planets and satellites, must certainly be the case); then a third capital motion will be introduced to this inhabitant of Saturn's satellite; and he will experience, in a narrow compass, what we now surmise may possibly be our case upon a more extended scale, by the motion of the whole system of stars to which our sun may belong. Another view may, perhaps, still better throw a light upon the subject. Let us admit that a very small nebula may be a collection of a thousand stars: and if Mr. MICHELL's opinion of our system of stars, which he assumes to be about a thousand (Phil. Transf. vol. LVII. p. 255.) has any foundation, all these stars taken together will only subtend an angle of barely a minute to an eye placed 3438 times as far from the center of the system as the two farthest stars in it are from each other. Now as I have found some of these nebulae that are so small, that a tolerably good telescope cannot distinguish them from a single star, whole systems of stars, when presented to our imagination under this diminutive shape of nebulae, will easily, I believe, be admitted among the number of those celestial bodies that may have a proper motion. I ought to carry this hint a little farther, just to shew that it may possibly be applied to the subject of resolving a number of concurrent proper motions of the fixed stars into apparent ones; and thereby, in process of time, to arrive at the knowledge of all the real complicated motions of the planet we inhabit; of the solar system

to

T A B L E I.

Names of stars.	Motion in R.A.	Motion in Decl.	Names of stars.	Motion in R. A.	Motion in Decl.
$\beta$ Ceti	+ 32		$\zeta$ Hydræ	- 23	
$\alpha$ Arietis	+ 10		$\gamma$ Leporis		- 10
$\delta$ Ceti	+ 15		$\epsilon$ Ursæ majoris	- 33	+ 10
$\alpha$ Ceti	+ 16		$\alpha$ Serpentarii	insens.	
$\alpha$ Persei	+ 16		$\gamma$ Draconis	+ 12	
$\eta$ Pleiadum		- 16	$\alpha$ Lyræ	insens.	+ 14
$\gamma$ Eridani	+ 14		$\gamma$ Aquilæ		- 20
$\epsilon$ Tauri		- 11	$\gamma$ Capricorni	+ 19	
$\alpha$ Aurigæ	+ 11	- 11	$\epsilon$ Pegasi		- 28
$\beta$ Orionis	insens.	insens.	$\delta$ Capricorni	+ 24	- 17
$\beta$ Tauri	- 11	- 13	$\alpha$ Aquarii	+ 13	
$\alpha$ Orionis	insens.	- 11	$\zeta$ Pegasi		- 13
$\mu$ Geminorum	- 16		Fomalhaut	+ 21	
$\epsilon$ Navis	- 13	- 11	$\beta$ Pegasi	+ 12	
$\beta$ Cancræ		- 14	$\alpha$ Andromedæ		- 21
$\epsilon$ Ursæ majoris	- 54		$\beta$ Cassiopeæ	+ 34	

to which it belongs; and even of the sidereal system, of which this sun may possibly be a member. We see then, that while the sun, by a proper motion, is going towards a certain point of the heavens, each of the stars belonging to the sidereal system, of which the sun is one, supposing them to be relatively at rest, with respect to each other, will be affected in the manner I have shewn (p. 261, &c. of the Paper on the proper Motion of the Sun) notwithstanding the whole system should have a real motion in absolute space, and change its situation with respect to other systems or intersystematical stars. We see also, that with respect to stars not belonging to our system, no parallax can appear but what is compounded of the proper motion of the sun, and of the whole system to which it belongs. And should there ever be found, in any particular part of the heavens, a concurrence of proper motions of quite a different direction, we shall then, perhaps, begin to form some conjectures besides those already mentioned by Mr. MICHELL (p. 253. of the same volume of Transactions) which stars may possibly belong to ours, and which to other systems.

TABLE

T A B L E II.

Polaris		+ 13	ζ Hydræ		+ 24
γ Ceti	- 14		α Hydræ		+ 13
β Persei	- 10		β Herculis	+ 14	
α Leporis		+ 11	γ Cygni	- 13	
μ Geminorum		+ 15	ε Pegasi	- 14	
ε Canis majoris		+ 10	ζ Pegasi	- 20	

From the first table we gather, that the principal stars, *Lucida Lyræ*, *Capella*,  $\alpha$  *Orionis*, *Rigel*, *Fomahand*,  $\alpha$  *Serpentarii*,  $\alpha$  *Aquarii*,  $\alpha$  *Arietis*,  $\alpha$  *Persei*,  $\alpha$  *Andromedæ*,  $\beta$  *Tauri*,  $\beta$  *Ceti*, and twenty more of the most distinguished of the second and third rank of stars, agree with our proposed solar motion; when, on the contrary, the second table contains but a few stars, and not a single one of the first magnitude amongst them to oppose it. It is also remarkable, that many stars of the first table agree both in right ascension and declination with the supposition of a solar motion, whereas there is not one amongst those of the second table which opposes it in both directions. This seems to indicate that the solar motion, in some of them at least, has counter-acted, and thereby destroyed the effect of their own proper motion in one direction, so as to render it insensible; otherwise it would appear improbable, that eight stars out of twelve, contained in the latter table, should only have a motion at rectangles, or in opposition to any one given direction. The same may also be said of nineteen stars amongst those of the former table, that only agree with the solar motion one way, and are as to sense at rest in the other direction; but these singularities will not be near so remarkable when we have the motion of the sun to compound with their

own proper motions. However, I forbear entering too much into refined consideration; what we are chiefly to determine at present is, an outline or sketch of what many repeated, and farther extended, observations must ripen so far as in time to enable us to apply more particular calculations.

The motions of  $\alpha$  Lyræ and  $\epsilon$  Ursæ majoris towards the north are placed in the first table; it will, therefore, be proper to shew the general law by which the apparent declinations of the stars, at present under consideration, are governed. Let an arch of 90 degrees be applied to a sphere representing the fixed stars, so as always to pass through the apex of the solar motion: then, while one end of it is drawn along the equator, the other will describe, on the spherical surface, a curve which will pass through the pole of the equator, and return into itself at the apex. This curve, to borrow a term from natural history, is a *non-descript* as far as I can find at present, and may be called a spherical conchoid from the manner of its generation. The law then is, that all the stars in the northern hemisphere, situated within the nodated part of the conchoid, will seem to go to the north by the motion of the solar system towards its apex; the rest will appear to go southwards. A similar curve is to be delineated in the southern hemisphere, in the nodated part of which the same appearances will take place. It will require but little attention to see the truth of this construction.

Suppose the great circle  $Acam$ , fig. 5. of which the generating quadrant  $mn$  is a part, compleated; then will it intersect the equator  $EQT$  in two opposite points  $me$ . Now, since the apex  $A$ , by the hypothesis, is somewhere north of the equator, the great circle will always make some angle  $AmQ$  with it; and the point  $n$ , which is 90 degrees from the intersection  $m$



with the equator, will be the most northern part of the semi-circle  $mnc$ . From what has been said (p. 261. of the paper on the Motion, &c.) it follows, that the apparent motion of any star  $sS$  will always be in an arch of a great circle  $AsSc$  drawn through the apex  $A$  and star  $sS$ : therefore, if the star be less than 90 degrees of the generating circle distant from its intersection with the equator (having more northern declination than the apex) as at  $s$ , its northern declination will increase, and it will also fall within the nodated part of the conchoid  $AxPy$ ; but when its distance from the intersection  $m$  is more than 90 degrees, as at  $S$ , the motion will be towards the south, and the star will be situated without the nodated part of the curve. That the star  $s$  will fall within the nodated part appears because  $ms$  being less than  $mn$  by supposition, if  $m$  be drawn towards  $E$ , to describe the conchoid the angle  $AmQ$  will decrease, and therefore the describing point  $n$  will be depressed below  $s$  as it approaches  $A$ . For the same reason  $S$  will fall without; since, by drawing  $m$  towards  $Q$ , the angle  $AmQ$  will become greater than  $SmQ$ , and the describing point  $n$  will pass above the star  $S$ . The application of this theory is very simple; for instance, let it be required to find whether any given star will fall within or without the conchoid. Then, in fig. 6, there will be given  $Ps$ , the polar distance of the star; and  $QPc$ , the difference of right ascension between the star and the apex of the sun's motion  $A$ ; also, the polar distance  $PA$ , and declination  $cA$  of the point  $A$ . Then, by trigonometry, the sides  $sP$ ,  $PA$ , and the included angle being given, we find the side  $As$  and angle  $PA s$ . Again, the side  $cA$ , and angle  $cAm = PA s$  of the right-angled triangle  $Acm$  being given, we find the hypotenuse  $Am$ ; and if  $Am + As$  be less than 90 degrees, the star falls within the conchoid, otherwise without.

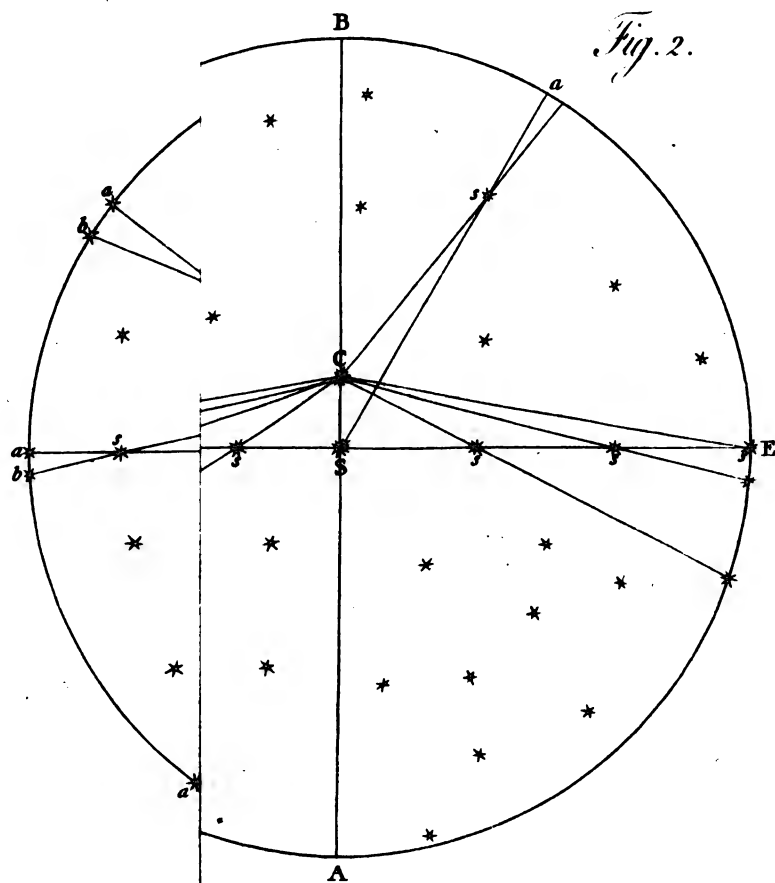
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It will be found, that I have placed the want of sensible motion of  $\alpha$  Lyrae and  $\alpha$  Orionis in right ascension, and of Rigel both in right ascension and declination to the account of those stars that are in favour. These stars are so bright, that we may reasonably suppose them to be among those that are nearest to us; and if they had any considerable motion, it would most likely have been discovered, since the variations of Sirius, Arcturus, Procyon, Castor, Pollux, &c. have not escaped our notice. Now, from the same principle of the motion of the solar system, by which we have accounted for the apparent motion of the latter stars, we may account for the apparent rest of the former. Those two bright stars,  $\alpha$  Lyrae and  $\alpha$  Orionis, are placed so near the direction of the assigned solar motion, that from the application of my second theorem (p. 261. of the paper on the Motion of the Solar System) their motion ought to be insensible in right ascension, and not very considerable in declination, all which we find is confirmed by observation. With respect to Rigel and  $\alpha$  Serpentarii, admitting them both as stars large enough to have shewn a proper motion, were their situation otherwise than it is, we find that they also should be apparently at rest in right ascension; and Rigel having southern declination, and being a less considerable star than  $\alpha$  Orionis, which shews but 11'' motion towards the south in 50 years, its apparent motion in declination may, on that account, be also too small to become visible.

I should not omit to take notice of a very remarkable paragraph of MAYER's, which seems to contain a strong objection against the motion of the solar system, while indeed it may be shewn to be a very good argument in its favour. At the end of his tract, De Motu Fixarum, he says: " Tandem, quum et quæri possit, quæ hujus motus causa sit, hoc unum monere visum,

“ illum explicare non posse per motum totius systematis so-  
 “ laris, et si nec impossibile sit, solem, ut ejusdem cum fixis  
 “ naturæ, instar harum quarundam in spatio mundano pro-  
 “ moveri. Nam si sol et cum ipso planetæ omnes nostrumque  
 “ domicilium terra, recta tenderent versus plagam aliquam, uni-  
 “ versæ fixæ, quæ in ea plaga adparent paullatim a se invicem  
 “ discedere, et quæ sunt in opposita parte coeli coire viderentur;  
 “ non secus ac per silvam ambulanti arbores, quæ ante viam  
 “ sunt, disjungi videntur, quæ a tergo, congregari.” Now, if  
 we recollect what has been said of the motion of the stars, we  
 find, that those, towards which I suppose the solar system to  
 move, do really recede from each other: for instance, Arcturus  
 from  $\alpha$  Lyræ;  $\alpha$  Aquilæ and  $\alpha$  Aquarii from  $\alpha$  Serpentarii and  $\alpha$   
 Ursæ majoris; and, on the contrary, those in the opposite part  
 of the heavens do really come nearer to each other; as Sirius  
 to Aldebaran; Procyon to  $\alpha$  Arietis; Castor, Pollux, Regulus,  
 &c. to  $\alpha$  Ceti,  $\alpha$  Persei,  $\alpha$  Andromedæ, &c. All this agrees  
 with what MAYER says ought to happen, if the solar system  
 was to have a motion towards a certain part of the heavens;  
 which, by the bye, I find this admirable astronomer mentions  
 as a very possible thing\*. However, when he says that *all*  
*the stars* in those parts towards which the sun might be sup-  
 posed to move, should recede from each other, and *vice versa*;  
 I must add, that this would only take place under the restric-  
 tions of my first, second, and third theorems, and therefore it  
 is not to be expected, that we should immediately see the effect  
 of this parallax in any but the stars that are nearest to us. But  
 as we have at present no other method of judging of the rela-  
 tive distance of the fixed stars than from their apparent bright-

\* This paper, De Motu Fixarum, was read at Gottingen in January 1760.

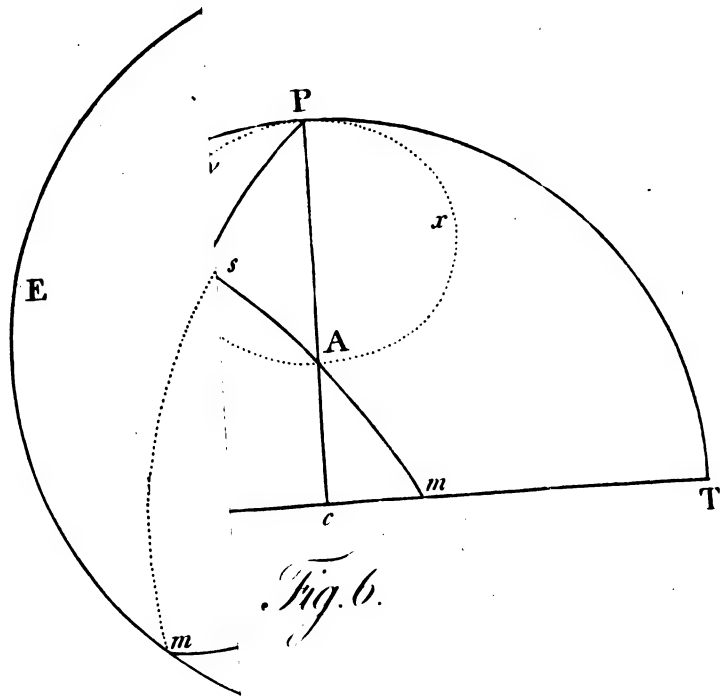








*Fig. 5.*



*Fig. 6.*





ness, those that are most likely, on that account, to be affected by a parallax arising from the motion of the solar system, are the very stars which, by MAYER's own table, I have made use of to point it out to us \*.

Datchet, March 13, 1783.

\* I have lately been favoured by Dr. WILSON, Professor of Astronomy at Glasgow, with a short tract, called, "Thoughts on general Gravitation, and Views thence arising as to the state of the Universe;" wherein the possibility of a Solar Motion is also shewn. It was printed in 1777. Mr. DE LA LANDE, in the Memoirs for 1776, with his usual felicity of thought, has inferred the probable motion of the system from the sun having a rotation round his axis, when he says, p. 513. "Une force quelconque imprimée à un corps, et capable de le faire tourner autour de son centre, ne peut manquer aussi de déplacer le centre, et l'on ne sauroit concevoir l'un sans l'autre. Il paroît donc très-vraisemblable que le soleil a un mouvement réel dans l'espace absolu," &c.

XVIII. *Some Experiments upon the Ochra friabilis nigra fusca of Da Costa, Hist. Foss. p. 102.; and called by the Miners of Derbyshire, Black Wadd. By Josiah Wedgwood, F. R. S.*

Read March 13, 1783.

**T**HE extraordinary circumstance of this substance taking fire upon being slightly mixed with linseed oil, first discovered by accident in the year 1752, at Mr. BASSANO's, a painter in Derby, has rendered it a subject of curiosity; but, as it is now employed in considerable quantities, and very advantageously, as an oil-colour in ship and house-painting, it has a better claim to our attention; and it is hoped, an attempt towards the further investigation of so curious and useful an earth may be acceptable to this illustrious Society.

It is many years since I first collected some of this earth, which balled out in a hollow way near Winster, in Derbyshire, and tried some experiments upon it; but as they were not very interesting to me at that time, and my hands being full of other matters, I made no further use of it till December last, when a series of experiments being made at the President's house upon its inflammable property when mixed with oil, at which JOHN WALSH, Esq. and several other gentlemen were present, Mr. WALSH was kind enough to send me a specimen from thence, and express his desire that I would analyse, and make some further experiments upon, this extraordinary substance.

Mr. WOODWARD, as well as Mr. DA COSTA, has described this earth so minutely, that it cannot easily be mistaken; but from the following experiments it will appear, that it should not be classed amongst the *Ochres not acted upon by acids*; and that it may, with as great propriety, be called *Manganese* as *Ochre*.

EXP. 1. Mixed with porcelain biscuit body, it gives darker or lighter shades of black and brown, as the quantity is greater or less in proportion to the body.

EXP. 2. Mixed with linseed oil, in the quantity of a few penny-weights only, into a paste, it dried very slowly, without producing any perceptible smoke or heat. The quantity, perhaps, was too small for ignition, and, I believe, it was over-dosed with oil.

EXP. 3. When the mineral was previously calcined with a slight red heat about half an hour, the mixture of it with the oil dried much sooner and harder; a circumstance which, if not already known, may render it still more valuable to the painter. In other respects no difference could be observed.

EXP. 4. In the above low heat it suffers no alteration of colour or texture. In a heat of  $30^{\circ}$ , by my thermometer for measuring high degrees of heat, it loses its property of staining the hands, diminishes very considerably in bulk, acquires a little hardness, though it still proves friable between the fingers, and has its colour changed from a brownish to a blueish black. In a heat of  $80^{\circ}$  it begins to melt; and at  $95^{\circ}$  runs into a black scoria.

EXP. 5. With black flux, in a heat of  $90^{\circ}$ , by the above-mentioned thermometer, it yielded a button of lead, amounting, in one experiment, to 21; and in another to 22 grains from an ounce, or nearly  $\frac{1}{12}$ .

EXP.

EXP. 6. Water extracts nothing from it. The mineral acids, with the assistance of heat, dissolve about eleven parts out of twelve; but a large quantity of acid is necessary for this solution. The residuum is greyish white, full of bright micaeous particles, with a few fine filaments like those of asbestos, which suffer no change in a moderate red heat. In a heat of  $144^{\circ}$ , which is  $14^{\circ}$  beyond the fusion of cast iron, it ran into a perfect glass; but whether this was a vitrification of the pure earth itself, or of a combination of it with the argillaceous matter it was in contact with, the smallness of the quantity did not admit of ascertaining. Upon the Hessian crucible it formed a black glass; what adhered to the thermometer-piece was brown.

EXP. 7. On boiling with oil of vitriol to dryness, the bottom and sides of the mass became red like colcothar, the middle white, the intermediate parts yellow or reddish yellow, and some greenish. These appearances were at first attributed to a vitriol of iron in different degrees of calcination; but, on separating some of the purer white and red parts, the former were found to produce in vitrification the same colour as manganese does, the latter the same as colcothar; the other seemed to be a mixture of the two.

EXP. 8. A solution of the mineral in nitrous acid was precipitated, instead of common alkali, with Prussian lixivium, which has the property of throwing down from acids iron, manganese, and all metallic bodies, but no one of the earthy class. When the addition of this lixivium ceased to make any further precipitation, common alkali, added afterwards, had also no effect; a proof that this mineral contains no earth soluble in acids, for that would have remained in the liquor after  
the

the precipitation of the other matters by the Prussian lixivium, and been precipitated by the alkali added at last.

EXP. 9. On precipitating a like solution by gradual additions of alkaline lixivium, and separating the precipitates as often as a fresh addition of the alkali occasioned any different appearance from what the preceding had done; the first precipitate was white; the next of a rusty red colour, like precipitate of iron; the last very white, while diffused through the liquor, and when settled, but in drying turned a little brown. The first, which was in a very small quantity, as nearly as could be judged by weighing the filters, about a twentieth part of the other two, was found to be lead; the second was iron; and the third manganese, nearly in equal quantities, all pure, or very nearly so, from one another.

It appears from these experiments, that 22 parts of this mineral contain nearly two of indissoluble earth, chiefly micaceous, 1 of lead, about  $9\frac{1}{2}$  of iron, and the same quantity of manganese.

*Specimens of the colours produced by vitrification.*

O. The mineral itself.

1. 2. 3. The first, second, and third precipitates.



XIX. *Mémoire sur la Manière de préparer, avec le moins de perte possible, le Sel fusible d'Urine blanc, et pur, et l'Acide phosphorique parfaitement transparent. By the Duke de Chaulnes; F. R. S. presented by Sir Joseph Banks, Bart. P. R. S.*

Read March 13, 1783.

**J**E désirais depuis longtems de m'occuper de ces objets, lorsque les circonstances me mirent, en 1773, à portée d'y employer le tems nécessaire et de me procurer facilement et en quantité les matériaux qui y sont indispensables.

Je crus devoir, avant tout, commencer par m'instruire des travaux qu'on avait entrepris sur ce sujet.

Parmi les anciens auteurs, RAYMOND LULLE, en avait dit quelque chose, sur le ton énigmatique et fastueux des rêveries alchimiques.

On trouve les manuscrits inconnus d'un vieux Allemand nommé TURNÉISSER, cités par M. POTT.

Le grand BOERHAAVE lui-même, avait dit sur ces substances quelques mots qui ne sont dignes de cet homme illustre, que relativement au tems où il écrivait.

Deux Allemands plus modernes et très-peu connus dans ce pays, appelés Mess. HAUPT et SCHLOSSER, s'en sont occupés depuis. Je me suis procuré la dissertation du dernier, mais je n'ai pû scavoir autre chose, des travaux de M. HAUPT, que ce qu'en ont rapporté Mess. POTT et MARGRAAF.

Ces deux derniers enfin, ont donné sur le sel fusible : le premier, une dissertation assez longue et fort obscure, traduite par M. DE MACHY : et le second un mémoire qui est ce que nous avons de mieux sur cette matière, et que l'on trouve dans ses opuscules chimiques, ainsi que deux autres, où il en est dit quelque chose relativement au phosphore, qui en est l'objet principal.

La foule d'observations qu'il rapporte, dénote un travail immense, mais bien confus ; je fus, surtout, étonné de trouver plusieurs faits faux, dans les ouvrages de ce savant, et j'oserais à peine l'avancer, si je n'étais à portée de le démontrer par des observations trop simples pour que je puisse être taxé de les avoir mal faites ; comme il est impossible de suspecter les connoissances de ces artistes célèbres, je crois qu'il vaut mieux s'en tenir à regretter qu'ils aient suivi la méthode, qui étoit ordinaire aux chimistes Allemands de ce tems, trop infectée d'idées alchimiques, et qui est non seulement très-embrouillée en général, mais qu'ils cherchent encore à rendre obscure, comme le dit très-bien la lettre suivante. Leur but paroît avoir été beaucoup plus de prouver qu'ils avaient des connoissances que de tendre à les communiquer. On trouve quelque clarté dans les faits, mais nuls détails dans la manière d'obtenir les résultats ; il faut soi-même travailler, avec peine, pour y parvenir. Il est vrai qu'on les trouve alors constans pour la plus part.

J'étais déjà venu à bout de me procurer du sel fusible et de l'acide phosphorique, lorsque je reçus la lettre qui suit de M. ROUELLE, célèbre chimiste de Paris.

“ Vous avez raison, Monsieur le Duc, de dire que M. MAR-  
“ GRAAFF est minutieux et qu'il ne s'explique guères, sur les  
“ moyens de préparer le sel fusible ; je crois qu'il en a fait un  
“ mystère, car il dit que son sel fusible, lorsqu'il est bien pré-



290 *Sur le Manière de préparer le Sel fusible d'Urine blanc, et pur,*

“ paré, donne par la distillation à la cornue de l’alkali volatil,  
“ et que l’acide phosphorique reste dans la cornue, sous la  
“ forme d’une matière transparente comme un verre ; de tous  
“ les chimistes qui ont parlé du sel fusible dans leurs écrits, je  
“ n’en fache aucun qui l’ait préparé tel que M. MARGRAFF le  
“ décrit ; si vous l’obtenez, vous Monsieur le Duc, conforme à  
“ sa description, je vous prie de m’en envoyer une ou deux  
“ onces ”

Je répondis à ce chimiste que j’avais obtenu le sel fusible composé d’acide phosphorique et d’alkali volatil ; que je n’en connoissais point d’autres, que s’il n’avait pas obtenu l’acide phosphorique transparent, c’était faute d’avoir exactement suivi la méthode indiquée par M. MARGRAFF, dans ses opuscules, c’est-à-dire de lui faire subir dans un creuzet un feu violent, après l’avoir tiré de la cornue.

J’envoyai en même tems à M. ROUELLE, les échantillons qu’il me demandait, et j’ai trouvé dans la suite qu’il en avait préparé, d’à-peu-près pareil à celui que je lui avais envoyé, mais que j’ai reconnu depuis pour être le sel fusible de première cristallisation, qui est très altéré par le sel marin, dont je vais parler, et dont le caractère principal est de ne pouvoir fournir le phosphore. Je fus toute-fois bien aisé de voir qu’on ne s’était point occupé de cette matière.

Pour achever de connaître les travaux qu’on avait pu entreprendre à ce sujet, je consultai Mess. MACQUER et BAUME dès que je fus de retour à Paris. Ils me confirmèrent dans l’opinion qu’il n’y avait point de procédés clairs et précis, pour obtenir le sel fusible ; M. BAUME me dit seulement qu’il s’en était procuré en faisant cristalliser de l’urine épaissie ; mais que lorsqu’il avait voulu procéder à une seconde cristallisation, tout avait semblé disparaître *comme par enchantement* ; je savois bien que c’était

un des principaux embarras de la preparation du sel fusible ; mon but est donc aujourd'hui, de donner avec le plus de clarté possible, les moyens de le préparer et de le conserver presque-entièrement, en lui faisant subir les purifications nécessaires. L'évaporation d'une grande quantité d'urine, étant le premier procédé pour avoir du sel fusible, je commençai par consulter les mémoires de M. MARGRAFF, et ce qu'en dit M. MACQUER très en abrégé dans son Dictionnaire de Chimie (edition de 1776). Le premier veut qu'on le fasse évaporer lentement dans des vaisseaux de terre bien vernissés, jusqu'à la consistance de sirop ; mais quoiqu'en général M. MACQUER ne parle que d'après M. MARGRAFF, comme il me l'a dit lui-même, il a cependant soin d'observer, d'après les évaporations d'urine, faites ici, qu'il est indifférent de la faire évaporer par une forte ou lente ébullition ; je rapporterai dans la suite du mémoire des faits qui prouvent que le sel fusible ne change de caractère, qu'à un degré de chaleur beaucoup plus fort.

Si l'on ne pouvait pas évaporer l'urine en la faisant bouillir, il serait bien difficile d'en faire évaporer 20 muids comme je l'ai fait alors.

Le lieu que je choisîs pour ce travail était un rempart écarté où j'avais fait placer six vieilles futailles couvertes, dans lesquelles un soldat invalide venait vuidier tous les matins des baquets d'urine fournie par une garnison de deux bataillons ; j'avais fait construire auprès de ces futailles un fourneau pareil à celui que décrit M. HELLOT, dans les Mémoires de l'Académie, pour l'année 1737. Le même soldat qui apportait l'urine dans les futailles conduisait l'évaporation pendant la journée, et m'en apportait le produit le soir à mon laboratoire ; le chaudron qui servait à évaporer tenait environ cinquantes pintes, et on le remplissoit trois fois par jour ; quand il ne restoit plus de la totalité,

252 *Sur la Maniere de preparer le Sel fusible d'Urine blanc, et pur,*  
lité, que trois ou quatre pintes d'extrait, on le ramassoit avec  
une cuillere de fer, dont le manche étoit perpendiculaire au euil-  
leron et dont la forme étoit exactement la même que celle du  
fond du chaudron. Comme l'urine gonfle prodigieusement,  
toutes les fois qu'on la chauffe de nouveau, ou lorsqu'on mêle de  
la nouvelle liqueur avec celle qui est déjà chaude, je trouvais  
plus simple, et je craignois moins le gonflement, en n'ajoutant  
qu'environ une pinte à chaque fois.

Lorsque j'évaporai l'urine de cette manière, je ne connoissais  
pas encore l'usage que M. BAUME fait des galeres; cette mè-  
thode est sans contredit la meilleure, je viens de m'en servir  
tout nouvellement, pour faire évaporer trois muids de cette  
liqueur, dans deux chaudrons de 150 pintes chacun, scellés sur  
un fourneau de cette espèce, construit avec de mauvais platras,  
en ne mettant dans chacun de ces deux chaudrons qu'une  
cinquantaine de pintes d'urine à la fois, et ne les enfonçant dans  
le fourneau que jusqu'au tiers de leur hauteur; de cette maniere,  
le gonflement n'est point à craindre, l'on est dispensé des atten-  
tions, et l'on consomme beaucoup moins de bois, pour évaporer  
la meme quantité d'urine.

Le commencement de l'évaporation (comme je l'ai déjà dit)  
n'est point difficile à conduire, et ne demande presque point  
d'autre soin que de veiller, au gonflement, soin dont l'on est  
même dispensé par le procédé qui précède, mais la fin en de-  
mande beaucoup, et qui sont même indispensables alors, si l'on  
veut obtenir le sel fusible presque pur dès sa premiere crystal-  
lisation.

L'urine est composée de parties salines extractives, et favo-  
neuses, les dernieres ne sont presque d'aucune considération dans  
la préparation du sel fusible, mais les premieres le sont si fort,  
que presque tout l'embarras du procédé ne vient que de la grande  
quantité

quantité de sel marin contenu dans l'urine, qui crySTALLISE très facilement, et pèle et mêle avec le sel fusible.

Le moyen que j'ai employé pour les séparer, et qui se présente naturellement à ceux qui connoissent la nature de ces sels, est celui de crySTALLISER le sel marin, par l'évaporation, et le sel fusible, par le refroidissement, mais ce moyen est sujet à plusieurs difficultés dans la pratique.

Non seulement la liqueur qui est épaisse et siropeuse filtre mal, mais les filtres seroient bientôt bouchés et la crySTALLISATION qui se ferait obstruerait tout, si l'on n'observoit diverses précautions pour s'en garantir.

Celles que j'emploie, sont d'interrompre l'évaporation dès qu'il commence à se précipiter du sel marin au fond de la liqueur, et de la faire passer alors par un tamis clair, en l'agitant toujours, pendant qu'elle est encore bien chaude. Ce procédé sépare tout le sel marin, qui s'est crySTALLISÉ par l'évaporation, et en débarrasse le sel fusible qui se crySTALLISE ensuite par le refroidissement.

Une seconde filtration est cependant nécessaire pour mieux dégager la liqueur des petites portions de sel marin, qui peuvent avoir passé par les interstices du tamis.

La manière dont je sépare ces petites portions de sel marin, est de filtrer la liqueur en la conservant la plus chaude qu'il est possible, et voici quel est le procédé que j'emploie pour cet objet : je soutiens un tonneau à environ un pied de distance de terre, à l'aide de quatre pieds de bois qui y sont ajustés ; le fond inférieur est percé d'un trou de six ponces de diamètre, par lequel je fais passer la pointe d'une chauffe de toile, soutenue par en haut au moyen de quatre bâtons croisés qui reposent sur les bords du tonneau, et le fond inférieur est doublé, en dedans, de tôle, pour pouvoir mettre de la braise dessus ; enfin l'on  
passé

294 *Sur la Maniere de preparer le Sel fasible d'Urine blanc, et pur,*  
passé une terrine sous la pointe de la chauffe, et l'on glisse sur le dessus du tonneau un couvercle qui, par ce moyen, le ferme au degré que l'on veut.

Il est facile de voir, d'après la description de cet appareil, qu'on est le maître du degré de chaleur qui règne dans l'intérieur du tonneau pendant la filtration, et qu'on prévient de cette manière la cristallisation qui embarrasseroit le filtre ; la liqueur qui passe dans la terrine, qui est froide, peut cristalliser sans nuire à l'opération, et on la porte alors à la cave, où on la laisse achever de cristalliser à volonté.

La principale et la vraie observation qui quoique simple, n'a été faite par aucun des auteurs qui ont parlé de cette cristallisation, est que si l'on a employé l'urine fraîche, le résidu est beaucoup plus épais et plus siropeux, que si l'on a commencé à laisser putréfier l'urine, quelques semaines avant l'évaporation, les résidus sont alors beaucoup plus clairs, se filtrent mieux, la séparation des sels est plus exacte, et ils cristallisent si aisément qu'ils le font quelque fois entièrement au bout de 24 heures, comme l'a fort bien remarqué le seul M. SCHLOSSER pendant que BOERHAAVE vouloit qu'on laissât évaporer pendant une année entière le résidu de l'urine évaporée.

Ces auteurs, au reste, ont bien rapporté leurs sentimens divers, mais n'ont point connu le principe d'ou procédoit la différence de leur avis : M. BAUME est le premier, que je sache, qui ait imprimé, qu'on pouvait conduire une cristallisation à son gré en épaississant plus ou moins la liqueur, avec une liqueur épaisse qui n'eut aucune action sur le sel qui y étoit contenu.

Quand la liqueur étant très froide, tout le sel est cristallisé contre les parois de la terrine, il faut prendre les précautions suivantes pour le retirer. On commence par verser à part la première moitié qui est la portion la plus claire, ensuite après  
avoir

et l'Acide phosphorique parfaitement transparent. 295

avoir détaché avec une spatule tout le sel adhérent à la terrine, on le verse avec la seconde sur un tervis de crin ordinaire, la liqueur entraîne par ce moyen toutes les saletés fines qui peuvent y être suspendues, et le sel reste sur le tamis.

On passe une seconde fois dessus de la même manière la première portion de la liqueur qui est la plus claire, qui se trouvant saturée ne dissout aucune portion de sel et la nettoye encore d'avantage.

Pendant que le sel fusible est encore humide de ces deux lutions, et après l'avoir ressuyé sur du papier gris, je procède à une troisième pour en lever la partie extractive par laquelle il est encore sali.

J'employois d'abord pour cet effet l'eau commune bien saturée à chaud de sel marin, et filtrée après son refroidissement, mais j'ai quitté cet usage parceque j'ai reconnu que cette eau, quoique bien saturée de sel marin, pouvoit encore dissoudre une bonne quantité de sel fusible; cette raison m'a déterminé à substituer dans le mortier où je mets mon sel fusible encore humide, de l'esprit de vin très rectifié à la dissolution saline dont je viens de parler: après avoir bien remué avec une spatule, cet esprit de vin avec le sel qui y est contenu, je verse le tout sur un tamis, comme auparavant; l'esprit de vin passe et suffit quoiqu'en très petite quantité pour enlever presque toute la matière colorante par laquelle le sel est terni, il est nécessaire pour que cette opération réussisse bien, d'avoir d'abord égrugé le sel fort menu, parce que c'est entre les lames des cristaux que sont contenues les saletés.

Le sel ayant été égoutté sur le papier gris est un peu plus blanc que le sel commun gris dans son état ordinaire, et peut être rendu alors parfaitement blanc par une seconde cristallisation, mais qui est sujette à bien des difficultés dont les auteurs

296 *Sur la Maniere de preparer le Sel fusible d'Urine blanc, et pur,*  
n'ont point fait mention, et sur lesquelles meme ils donnent le  
change.

M. MARGRAAFF conseille pour procéder à cette seconde cry-  
stallisation du sel fusible, de le dissoudre tout simplement dans  
l'eau chaude, de le laisser cristalliser, d'évaporer ensuite ce qui  
reste de liqueur, on retire encore un peu de sel, dit-il par la  
cristallisation ; enfin il ajoute qu'en répétant 3. ou 4. fois ce  
procédé, on obtient le sel fusible parfaitement blanc et pur.

Je ne fais comment M. MARGRAAFF l'entend, mais ce qu'il  
y a de certain, c'est que si on répétoit trois ou quatre fois ce  
procédé, on ne retireroit pas un atôme de sel fusible ; si l'on en  
fait dissoudre à la fois seulement une livre et demie ou deux, on  
en retire avec peine le tiers de la premiere quantité, et quand  
une fois on a fait évaporer le restant de la dissolution, à peine  
en retire-t-on quelques atomes, c'est ce qui est arrivé à M.  
BAUME' lorsqu'ayant obtenu par une premiere cristallization 8  
ou 10 livres de sel fusible, à peine a t'il pû en conserver quel-  
ques onces par une seconde.

Je n'ai pas trouvé ce phénomène tout à fait aussi outré qu'on  
l'assure, mais il est très certain qu'on retire à grande peine le  
tiers du sel dissous et même beaucoup moins, si on veut le pu-  
rifier à grandes doses ; la raison de cette deperdition est pous-  
sant fort extraordinaire, le sel fusible tel que M. MARGRAAFF le  
décrit, et tel que je l'ai obtenu, est un composé mi-partie d'al-  
kali volatil et d'acide phosphorique, et il seroit assez simple que  
l'alkali volatil ne conservat que la plus foible adhérence avec  
l'acide phosphorique qui est très fixe ; mais comment cet acide  
phosphorique si fixe au feu est-il si facilement entraîné par l'al-  
kali volatil, qui lui est uni si foiblement et qui est d'une nature  
si différente ?

Je

Je vais rapporter les preuves que j'en ai et les diversës expériences que j'ai faites à ce sujet, avant de passer aux moyens que j'ai trouvés pour lui procurer presque sans perte les derniers degrés de pureté.

Je crois devoir commencer, pour donner des idées nettes sur le sel fusible, par rapporter la décomposition que j'en ai faite, qui n'est guères que la répétition du procédé qu'indique M. MARGRAAFF pour cet objet.

J'ai pris 4 onces de sel fusible purifié par l'esprit de vin que j'ai distillé à sec, dans une petite cornue de verre au bain de sable, toute la masse s'est fondue, a bouillonné et il a passé dans le récipient une once d'alkali volatil fluor: quand il n'est plus sorti de liqueur, j'ai ôté la cornue et l'ayant cassée, j'en ai retiré une once de scories blanchâtres, que j'ai mises dans un creuset auquel j'ai fait effluer le feu le plus violent; elles se sont fondues en un fluor transparent: je les ai coulées alors sur une plaque de fer poli, où elles ont pris l'apparence de la plus belle topaze, qui est devenue en se refroidissant aussi blanche et aussi transparente que le plus beau stras.

J'ai été curieux d'éprouver quelles seroient les différences du sel fusible de première cristallisation par le même procédé, c'est à dire de celui qui contenoit nécessairement beaucoup plus de parties extractives et de sel marin, et à quel point étoit vrai ce que disoit M. MARGRAAFF, du procédé employé par M. HAUPT, pour purifier ce sel par la violence du feu.

J'ai pris en conséquence une livre de sel fusible, tel qu'il sort de l'urine \* et simplement lavé, j'en ai retiré par la cornue 8 onces

\* J'ai répété une autre fois cette expérience et j'ai observé une circonstance bien singulière, dont je n'ai pu pénétrer la cause, vers le milieu de la distillation, il s'éleva dans toute la cornue de petites parcelles brillantes qui voltigeoient sans se



298 *Sur la Maniere de préparer le Sel fusible d'Urine blanché, et pur,*  
 onces d'alkali volatil fluor, et il m'est resté 7 onces et demie de  
 scories, un peu plus moussantes que les premières. J'ai mis les  
 scories dans un creuset, et les ayant fondues, je les ai coulées  
 de même sur la plaque de fer poli; au bout d'un quart d'heure  
 de fusion, elles étoient transparentes dans le creuset, mais elles  
 ont à peine été figées sur la plaque de fer, qu'elles sont devenues  
 blanches et opaques comme de l'email blanc, et ont exhalé  
 une forte odeur d'acide marin. Je voulus voir s'il seroit possible  
 de les en purifier par un feu violent, comme le dit M. HAUT  
 et je les remis dans un creuset où je les laissai éprouver pendant  
 une heure une bonne fusion, l'odeur d'acide marin continua  
 toujours à s'exhaler du creuset, mais lorsque je les coulai au  
 bout de ce tems sur la plaque de fer poli, elles y restèrent abso-  
 lument transparentes en se refroidissant, et conserverent une aussi  
 belle apparence que le premier acide phosphorique; mais il est  
 bien éloigné d'avoir le même degré de pureté et M. MAR-  
 GRAFF à raison quand il dit que le sel préparé par M. HAUT  
 de cette façon n'est point le sel fusible tel qu'il le décrit.

Effectivement il y a plusieurs différences frappantes: le der-  
 nier acide phosphorique du sel fusible impur, est bien aussi  
 transparent que celui qui sert de base au sel fusible purifié lors-

qu'il se fixe et qui avoient l'apparence de particules de verre soufflé, il s'en sublimoit une  
 portion à la voute de la cornue, mais il n'en retombait d'avantage sur le résidu, et  
 cette dernière partie commençoit à s'y dissoudre, ce qui me força d'interrompre la  
 distillation. Pour recueillir cette sublimation singulière; je detachai donc avec les  
 barbes d'une plume les legeres parcelles qui ressembloient à du sel sédatif sublimé,  
 et je les mis à part.

Je n'ai trouvé qu'une observation semblable à celle-ci, dans les observations sur  
 l'urine de vache que M. ROUELLE a données et qu'on trouve dans le Journal de  
 Medecine de Novembre, 1773. il y parle d'un sel acide volatil qui se sublime en  
 parcelles brillantes comme des fleurs de benjoin, et par conséquent comme celui  
 que j'ai obtenu.

qu'il est nouvellement fait, mais en le laissant exposé à l'air il s'y humecte promptement et y devient opaque; son degré de déliquescence et d'opacité, denote la quantité d'acide marin qu'il a retenu et qui change entierement sa composition.

L'acide phosphorique du sel fusible pur ou de seconde cristallisation, immédiatement mêlé avec de la poudre de charbon ou toute autre substance contenant du phlogistique, fournit du phosphore; j'en ai retiré par l'intermede du zinck et l'acide phosphorique de sel fusible de premiere cristallisation ou de celui qui contient du sel marin n'en fournit pas par le même procédé.

L'odeur d'acide marin qui se dégage du creuset, pendant la fusion de ce dernier, nous apprend ce qui se passe durant cette opération. L'acide phosphorique, s'unissant à l'alkali fixe minéral, qui sert de base au sel marin, forme avec lui un nouveau sel fusible, et dégage l'acide marin qui étant devenu libre s'échappe en vertu de sa volatilité.

Le sel qui résulte de la purification de M. HAUPT, est donc très différent du sel fusible ammoniacal, décrit par M. MAR-GRAAFF, qui est le seul qui donne le phosphore, puis qu'il est réellement un sel fusible à base d'alkali fixe, que l'on obtient, exactement pareil, en unissant l'alkali minéral à l'acide phosphorique purifié.

L'acide phosphorique que je vis parfaitement transparent, à mon retour du Havre, chez le chimiste qui m'avait écrit, et que je retrouvai opaque peu de tems après, doit donc être naturellement du sel fusible obtenu par la simple cristallisation de l'urine, et auquel on n'a point fait éprouver la seconde cristallisation, qui en fait effectivement toute la difficulté.

La dernière expérience sur le sel fusible impur que je viens de rapporter, m'apprenait un fait fort singulier. Le sel fusible de seconde cristallization etoit, ainsi que le premier, composé de  
particules

300 *Sur la Maniere de preparer le Sel fusible d'Urine blanc, et pur,*  
 particules d'alkali volatil et d'acide phosphorique : on pouvait  
 seulement supposer dans celui de premiere crySTALLISATION, une  
 portion de sel marin, de plus, dont on n'avoit encore pu le dé-  
 pouiller, mais le sel marin étant fixe lui même, comment se  
 faisoit il qu'on ne pût retirer qu'à grande peine moitié du poids  
 du sel fusible de premiere crySTALLISATION, lorsqu'on voulait le pu-  
 rifier; par une simple dissolution dans l'eau chaude, et une se-  
 conde crySTALLISATION; et pouvoit-il régénérer un sel fusible ammo-  
 niacal, comme on me l'avoit dit, en y ajoutant de l'alkali volatil  
 coneret. J'en fis l'expérience, et après avoir retiré tout le sel  
 fusible qui voulut crySTALLISER, après avoir dissout dans l'eau  
 bouillante, et filtré du sel fusible impur, j'ajoutai l'alkali volatil  
 coneret \* dans la dissolution qui restait; j'eus une vive effe-  
 vescence et je comptais obtenir encore beaucoup de sel, mais à  
 ma grande surprise, je n'en obtins pas beaucoup plus que je n'en  
 aurois eû sans cette ressource; je résolus alors de m'éclaircir par  
 un procédé certain, du sort qu'éprouvait l'acide phosphorique  
 dans cette seconde crySTALLISATION. Je fis en conséquence dis-  
 soudre quatre onces de sel fusible de premiere crySTALLISATION dans  
 l'eau froide qui fut nécessaire pour le tenir en dissolution, et  
 ayant distribué cette liqueur dans 4 assiettes de verre, que j'eus  
 soin de couvrir de gaze, je laissai crySTALLISER et sécher la totalité,  
 puis l'ayant pesé je ne retrouvai qu'une once 6 gros 22 grains;  
 d'où pouvait venir cette étrange diminution, de plus d'une once à  
 la simple chaleur d'un cabinet où le thermometre de M. DE  
 REAUMUR n'a pas monté plus haut que dix degrés au dessus de  
 glace?

\* Ce sel a une singularité, c'est que si l'on évapore la dissolution de son acide  
 phosphorique séparé de nouveau de son alkali volatil cet acide phosphorique du sel  
 régénéré, crySTALLISE, pendant que celui du sel fusible ordinaire reste en gelée.

Ce

Ce fait ne constatoit pas moins la certitude de cette étrange évaporation, dès lors il étoit naturel que la chaleur de l'eau bouillante l'augmentât beaucoup, et que pendant la dissolution et la cristallisation, il se perdit la plus grande partie du sel fusible.

Je tentai encore un moyen que j'imaginai qui pourroit me procurer une seconde cristallisation sans perte ; ce fut d'essayer de faire la dissolution et la cristallisation dans des vaisseaux clos.

Je mis pour cet effet 4 onces de sel fusible impur dans un entonnoir garni de papier gris, soutenu par des pailles, et je portai l'entonnoir sur un bocal, que je mis sur une plaque de verre, puis je couvris le tout avec un récipient, ouvert par en haut, que je lutai à la même plaque avec de la cire molle, et par le trou duquel je versai ensuite sur le sel fusible de l'eau bien bouillante, je bouchai, ensuite promptement le trou du récipient, et je me flattais de prévenir par ce moyen toute déperdition mais il n'y eut qu'une très légère dissolution de la substance, ce qui devait arriver dans des vaisseaux bien clos.

Je m'avais enfin d'un autre expédient dont la réussite fut si parfaite, que je puis par son moyen retirer 4 livres de sel fusible parfaitement blanc et pur, de cinq livres de sel fusible, de première cristallisation ; voici le procédé.

Je fais chauffer dans un petit matras, à long col, le sel fusible, gris, lavé à l'esprit de vin, que je veux faire dissoudre jusqu'à ce que la surface commence à fariner, je verse alors dessus, la moitié de son poids, d'eau distillée, bien bouillante, que j'ai fait chauffer dans une fiole à médecine, je la fais bouillir encore un moment dans le matras, avec le sel fusible, jusqu'à ce que je m'aperçoive qu'il est entièrement dissout ; je le verse ensuite promptement dans un entonnoir, garni de papier gris, et posé sur une fiole à médecine, la dissolution, n'étant faite qu'avec une quantité d'eau égale à la moitié du poids du sel,

sel, se figeroit sur le champ et ne pourroit se filtrer, si l'on n'avoit recours à l'expédient suivant : j'emploie une fiole à médecine à laquelle j'attache une ficelle pour la soutenir au dessus du feu et entretenir bouillante, l'eau bouillante, dont je la remplis, je la pôle en cet état sur l'intérieur de l'entonnoir, et il en résulte deux effets ; la panse ronde de la fiole s'appliquant exactement contre les parois de l'entonnoir, par le moyen du papier gris empêche l'évaporation, et la chaleur de la masse, entretient celle de la liqueur, qui par ce moyen filtre bien, passe toute entière et ne peut boucher le filtre en cristallisant. Quand elle a achevé de passer entièrement par la filtration dans la bouteille inférieure, qui est enterrée dans un bain de sable échauffé, de 30 à 40 degrés à peu près, pour prévenir la cristallisation pendant le passage de la liqueur, je laisse le tout refroidir très lentement, et il arrive, seulement, alors, que les quatre cinquièmes environ du sel fusible cristallisent. Le peu de liqueur, qui reste sur les cristaux retient la petite quantité de sel marin, qui a pu échapper aux précautions que l'on a prises, dans le tems de la première cristallisation.

Ce sel fusible peut, par ce procédé, soutenir l'épreuve à laquelle je le soumets pour connoître son degré de pureté, qui est d'en mettre quelques grains dans une verre et de verser dessus des gouttes d'huile de vitriol bien blanche et bien concentrée ; s'il ne s'en dégage pas d'odeur d'acide marin, c'est une preuve qu'il est bien séparé de tout sel de cette nature ; mais il faut bien prendre garde à n'employer que l'acide le plus blanc et des petits bâtons de verre, ou d'émail pour l'agiter, car si l'on employoit de l'acide un peu phlogistique, ou des pailles au lieu de verre pour remuer l'acide, quoiqu'il fut pur, ou aurait inmanquablement une odeur d'acide sulfureux, qu'il est très aisé de confondre lorsqu'on fait les expériences en petit, avec celle de l'acide marin.



*\*XX. Experiments for ascertaining the Point of Mercurial Congelation. By Mr. Thomas Hutchins, Governor of Albany Fort, in Hudson's Bay †.*

Read April 10, 1783.

**T**HE following experiments, to determine the freezing point of quicksilver, were made by the direction of the Royal Society, at Albany Fort in Hudson's Bay, situated in the latitude of  $52^{\circ} 14'$  North and  $82^{\circ}$  West longitude from Greenwich.

The instruments used in these experiments were simply thermometers, except the apparatus F and G, furnished by Mr. CAVENDISH, and of these a more satisfactory idea will be formed from the annexed drawing than could be conveyed by words alone; I have, therefore, only specified a few particulars, so that each instrument may be distinguished from another.

I have compared the instruments with each other for several weeks in the various temperatures, to adjust, with the greater precision, the relative degrees on the scales; which was the more necessary as they differed very much.

The five first experiments were made exactly according to the directions sent to me by the Society, in order to obtain the point of congelation. The two succeeding ones are also made in the manner they directed, to endeavour to ascertain the greatest degree of contraction mercury is capable of; then follow two

† This paper having been for some time mislaid, could not be printed in its turn. This accounts for the double paging and signatures.

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\*S 1

experiments

experiments made in a different manner by my own suggestion; and, lastly, an account of mercury frozen in the open air without the aid of any artificial cold, which will be found to corroborate the preceding experiments, and determine the exact point of congelation to be at  $40^{\circ}$  below the cypher. I have been careful to mark down every circumstance attending the experiments, and have added a few observations to each of them, to elucidate any uncommon phænomena that occurred.

If these experiments should be agreeable to the Royal Society, the merit must be attributed to the excellent instructions they transmitted to me, which left me nothing to do but to follow them; yet I cannot avoid doing justice to the ingenious Dr. BLACK, Professor of Chemistry at Edinburgh, who favoured me with some remarks on the experiments I made in 1775 to freeze quicksilver, and first suggested this method of ascertaining the point of congelation, which I had the honour of communicating to the Royal Society by the means of SAMUEL WEGG, esq. whose attention to promote the views of the Society can only be equalled by that liberality of sentiment and great goodness of heart which makes him encourage even the most feeble attempt than can in the least tend to the improvement of knowledge.

THOMAS HUTCHINS.

*Dr.*

*Dr. BLACK's Letter referred to above.*

DEAR SIR,

Edinburgh, 5th Oct. 1779.

I HAVE read with great pleasure the experiments made at Hudson's Bay, upon the congelation of mercury, and observe that the author has succeeded perfectly in effecting it; but could not determine with precision what degree of cold was necessary to produce it. This, however, does not surprize me, as I have always thought it evident, from Professor BRAUN's experiments, ~~that~~ this degree of cold cannot be discovered conveniently by congealing the mercury of the thermometer itself. I shall not here ~~give my reasons for this opinion; they~~ would lengthen out this letter too much; I shall only propose what appears to me the proper manner of making the experiment, which is as follows: provide a few wide and short tubes of thin glass, sealed at one end and open at the other; the wideness of these tubes may be from half to three-quarters of an inch, and the length of them about three inches. Put an inch or an inch and an half depth of mercury into one of these tubes, and plunging the bulb of the thermometer into the mercury, *set the tube with the mercury and the thermometer in it* into a freezing mixture, which should be made for this purpose in a common tumbler or water-glass; and, N. B. in making a freezing mixture with snow and spirit of nitre, the quantity of the acid should never be so great as to dissolve the whole of the snow, but only enough to reduce it to the consistence of Panada. When the mercury in the wide tube is thus set in the freezing mixture, it (the mercury) must be stirred gently and  
\*S f 2 frequently



\*306 *Mr. HUTCHINS's Experiments for ascertaining*

frequently with the bulb of the thermometer; and if the cold be sufficiently strong, it will begin to congeal by coming thick and broafy like an amalgam. As foon as this is obferved, the thermometer fhould be examined without lifting it out of the congealing mercury; and I have no doubt, that in every experiment, thus made, with the fame mercury, the instrument will always point to the fame degree, provided it has been made and graduated with accuracy.

I am, DEAR SIR,

Your faithful humble fervant,

JOSEPH BLACK.

To Mr. ANDREW GRAHAM, Edinburgh.

*Thermometers*

*Thermometers described;*

- A. Represents a mercurial thermometer, with an air-bulb at the top, graduated 628 degrees below the cypher, and marked at every second degree. Makers NAIRNE and BLOUNT; the scale box-wood.
- B. Another mercurial thermometer graduated to 526° below the cypher, each line representing 2°, made by NAIRNE and BLOUNT; the scale box-wood.
- C. Is a fine mercurial thermometer, with an air-bulb at the top graduated 2300° below the cypher, each division containing 5°; the scale made of box, by THROUGHTON.
- D. A small spirit thermometer on a box scale, made by THROUGHTON, and divided to every single degree down to 160° below the cypher.
- E. Another spirit thermometer, by the same maker (THROUGHTON) graduated 90° below the cypher; the scale box.
- F. A small mercurial thermometer, on an ivory scale, divided at every 5° between 220° above and 250° below the cypher; made by NAIRNE and BLOUNT.
- G. Another mercurial thermometer, every way like the last mentioned, except only reaching from 215° above to 250° below the cypher; by NAIRNE and BLOUNT.
- H. A spirit thermometer, made by NAIRNE and BLOUNT, with which I have made meteorological observations from the year 1774.

Thermometers compared.

**Thermometers compared.**

Year, Month, and Day.	Hours.	A.	B.	C.	D.	E.	F.	G.	H.	
1782 Jan. 16	8 AM.	-17	-16	-12	-7	-8	-14½	-15	-20	
	Noon	-7	-5	-4	+1	-0	-4	-4	-11	
	4 PM.	-6	-6	-5	-0	-1	-6	-6	-11	
17	8 AM.	-14	-12	-11	-4	-5	-9	-9	-16	
	Noon	-6	-4	-2	+2	+1½	-3	-3	-10	
	4 PM.	+2	+4	+6	+8½	+7½	+5	+5	-2	
	8 PM.	-4	+2	-1	+4	+3	-1	-1	-5	
18	8 AM.	-9	-8½	-5	-0	-1	-6	-6	-11	
	Noon	-2	-0	+2	+4	+4	-0	-0	-6	
	4 PM.	-8	-7	-5	-1	-2	-6	-6	-10	
	8 PM.	-15	-13	-11	-5	-6	-11	-11	-16	
19	8 AM.	-12	-10	-7½	-4	-4	-9	-9	-14	
	Noon	-8	-6	-4	-0	-1	-5	-5	-12	
	4 PM.	-11	-10	-7	-3	-4	-8	-8	-12	
	8 PM.	-20	-18	-15	-9	-8	-11	-11	-20	
20	8 AM.	-12	-10	-7	-3	-3	-8½	-8½	-13	
	Noon	-12	-10	-7	-3	-4	-8	-8	-14	
	4 PM.	-16	-14	-12	-7	-6	-13	-13	-18	
	8 PM.	-21	-20	-17	-10½	-11	-17	-17	-22	
21	8 AM.	-38	-36	-33	-24	-25	-38	-38	-37	
	Noon	-28	-26	-23	-16	-17	-25	-25	-30	
	4 PM.	-22	-20	-17	-12	-13	-18	-18	-24	
	8 PM.	-28	-26	-24	-16	-16½	-24	-24	-28	Therm. C broke this day.
22	Noon	-19	-17		-8½	-9	-15	-15	-21	
	4 PM.	-16	-14		-7	-8	-13	-13	-18	
	7 PM.	-20	-18		-9	-10	-17	-17	-22	
23	8 AM.	-34	-32		-21	-21½	-30	-30	-34	
	Noon	-16	-14		-6½	-7	-13	-13	-19	
	4 PM.	-12	-10		-4	-5	-9	-9	-14	
24	8 AM.	-14	-12		-4½	-5	-10½	-10	-16	
	Noon	-4	-2		+3½	+3	-1	-1	-7	
	4 PM.	+2	-0		+5½	+4	+2½	+2	+4	
	8 PM.	-12	-10		-3½	-4	-8	-8	-14	
25	8 AM.	-30	-28		-18	-18½	-25½	-25	-30	
	Noon	-30	-28		-17½	-18½	-25	-25	-30	

Thermometers

Thermometers compared.

Year, Month, and Day.	Hours.	A.	B.	D.	E.	F.	G.	H.	
1782									
Jan. 26	8 AM.	-103	-80	-33½	-33	-42½	-42	-46	{ Quickfilver froze in air.
	9 AM.	-323	-444	-29	-29½	-40	-40	-44	
	Noon	-34	-32	-21	-21½	-30	-29½	-34	
	4 PM.	-30	-28	-17	-18	-25	-25½	-32	
	8 PM.	-38	-36	-24	-24	-35	-34	-36	Quickfil. not frozen.
27	8 AM.	-44	-42	-29½	-30	-40	-40	-43	
	Noon	-28	-26	-16	-17	-24	-24	-28	
	4 PM.	-26	-24	-14	-14½	-21	-21	-26	
	8 PM.	-30	-28	-17	-18	-25	-25	-29	
28	8 AM.	-30	-28	-17½	-18	-25	-25	-30	
	Noon	-20	-18	-10½	-11	-17	-17	-22	
	4 PM.	-22	-20	-12	-13	-18½	-19	-23	
	8 PM.	-26	-24	-14	-14½	-22½	-23	-27	
29	8 AM.	-38	-36	-24	-24½	-34	-34	-37	
	Noon	-32	-30	-19	-20	-27	-27	-32	
	4 PM.	-30	-28	-17	-18	-25	-25	-30	
30	Noon	-24	-22	-13	-14	-21	-21	-26	
	4 PM.	-26	-24	-14	-15	-20	-20	-26	
	8 PM.	-28	-26	-16	-16	-24	-23½	-28	
31	8 AM.	-34	-32	-20	-21	-28	-28	-33	
	Noon	-24	-22	-13	-14	-20	-20	-26	
Feb. 1	8 AM.	-38	-36	-24	-24½	-34½	-35	-38	
	Noon	-28	-26	-16	-17	-25	-25	-9	
	4 PM.	-22	-20	-12	-13	-19	-19½	-24	
2	8 AM.	-30	-26	-17	-18	-24½	-25	-29	
	Noon	-20	-18	-9	-10	-15	-15½	-21	
	4 PM.	-21	-19	-10	-10½	-16	-16½	-22	
	8 PM.	-22	-20	-10	-10½	-16	-16½	-22	
3	8 AM.	-18	-16	-9	-10	-15	-15	-20	
	1 PM.	-4	-2	+ 4	+ 3	-1	-1	-8	
	8 PM.	-13	-11	-4	-5	-8	-8	-14	
4	8 AM.	-12	-10	-3	-4	-7	-7	-12	
	Noon	-8	-6	-0	-0	-4	-4	-10	
5	8 AM.	-32	-30	-19	-20	-26½	-27	-32	
	Noon	-16	-14	-6	-7	-13	-13	-18	
	4 PM.	-12	-10	-3	-4	-7	-7½	-14	
	8 PM.	-12	-10	-2	-3	-7	-7	-14	

Thermometers

Thermometers compared:

Year, Month, and Day.	Hours.	A.	B.	D.	E.	F.	G.	H.	
1782									
Feb. 6	8 AM.	- 4	- 2	+ 3	+ 2	- 1	- 1	- 6	
7	8 AM.	- 34	- 32	- 20	- 21	- 28	- 28	- 32	
	4 PM.	- 14	- 12	- 5	- 6	- 10	- 10½	- 16	
	8 PM.	- 16	- 14	- 6	- 7	- 11	- 11	- 17	
8	8 AM.	- 10	- 8	- 2	- 3	- 6	- 6½	- 12	
	Noon	- 10	- 8	- 1	- 2	- 5½	- 6	- 12	
	8 PM.	- 20	- 18	- 9	- 10	- 14½	- 15	- 20	
9	7 AM.	- 24	- 22	- 14½	- 15	- 20	- 20½	- 26	
	Noon	- 22	- 20	- 11	- 12	- 17½	- 18	- 23	
	8 P.M.	- 29	- 26	- 16	- 17	- 24	- 25	- 28	
10	8 AM.	- 14	- 12	- 3½	- 4	- 9	- 9½	- 15	
	Noon	- 0	+ 2	+ 6	+ 5	+ 3	+ 2½	- 4	
	4 PM.	+ 2	+ 4	+ 7	+ 7	+ 5	+ 5	+ 2	
	8 AM.	- 2	- 0	- 6	- 5½	- 3	- 3	- 4	
11	8 AM.	- 24	- 22	- 12	- 13	- 19	- 19½	- 24	
	Noon	- 18	- 16	- 8	- 9	- 14	- 14½	- 19	
	4 PM.	- 14	- 12	- 5	- 5	- 10	- 10	- 16	
	8 PM.	- 24	- 22	- 12	- 13	- 19	- 19½	- 24	
12	8 AM.	- 2	- 0	+ 5	+ 4	+ 1	- 0	- 5	
	Noon	+ 8	+ 10	+ 14	+ 13	+ 11	+ 10½	+ 4	
	4 PM.	- 10	- 12	- 16	- 15	- 14	- 14	- 7	
	8 PM.	- 4	- 2	- 10	- 11	- 7	- 7	- 0	
13	8 AM.	- 15	- 12	- 4	- 5	- 10	- 10	- 15	
	Noon	- 12	- 10	- 1	- 2	- 5	- 5	- 13	
	4 PM.	- 6	- 4	+ 1	- 0	- 4	- 4	- 9	
	8 PM.	- 12	- 9	- 3	- 2½	- 7	- 6	- 12	
14	8 AM.	- 2	- 0	+ 6	+ 5	- 4	- 3	- 5	
	4 PM.	- 2	- 1	- 6	- 3	- 4	- 3	- 5	
15	Noon	- 10	- 8	- 1	- 2	- 5	- 5½	- 12	
	4 PM.	- 8	- 6	- 2	- 5	- 5	- 11	- 4	
16	Noon	- 0	+ 2	+ 7	+ 6	+ 4½	+ 4½	- 3	
	4 PM.	+ 4	+ 6	+ 10½	+ 9½	+ 8	+ 8½	+ 2	
	8 PM.	+ 6	+ 4	+ 13	+ 12	+ 7	+ 7	- 4	
17	8 AM.	+ 12	- 14	- 17	- 16	- 15	- 15	- 8	
	1 PM	- 6	- 4	- 11	- 10	- 9½	- 9	- 3	
	4 PM.	- 4	- 2	+ 4	+ 3	0	0	- 6	
	8 PM.	- 8	- 6	- 0	- 0	- 5	- 5	- 9	

Thermometers compared.

Year, Month, and Day.	Hours.	A.	B.	D.	E.	F.	G.	H.
1782 Feb. 18	8 AM.	-12	-10	-2	-3	-5 $\frac{1}{2}$	-6	-12
	Noon	-10	-8	-1	-2	-5	-5 $\frac{1}{2}$	-12
	8 PM.	-22	-20	-11	-11	-16 $\frac{1}{2}$	-17	-22
19	8 AM.	-34	-32	-21	-21 $\frac{1}{2}$	-29	-29	-33
	Noon	-18	-16	-7	-8	-13	-13 $\frac{1}{2}$	-19
	4 PM.	-12	-10	-3	-4	-8	-8	-14
	8 PM.	-19	-17	-8	-8 $\frac{1}{2}$	-14	-14	-20
20	8 AM.	-18	-16	-7 $\frac{1}{2}$	-8	-13	-13 $\frac{1}{2}$	-19
	Noon	-0	+2	+7	+6	+4	+3 $\frac{1}{2}$	-4
	4 PM.	-6	-4	+2	+1	-1	-1 $\frac{1}{2}$	-7
	8 PM.	-8	-6	-0	-1	-4	-4	-10
21	8 AM.	-30	-28	-17	-18	-25	-25	-30
	Noon	-29	-27	-16 $\frac{1}{2}$	-17	-24	-24 $\frac{1}{2}$	-29
	4 PM.	-28	-26	-17	-17	-23 $\frac{1}{2}$	-24	-28
	8 PM.	-34	-32	-20	-21	-27	-27 $\frac{1}{2}$	-32
22	7 AM.	-82	-66	-34	-34	-42	-42	-46
	1 PM.	-35	-32	-21	-22	-30	-30	-34
	4 PM.	-34	-32	-21	-22	-30	-30	-34
	8 PM.	-39	-38	-24 $\frac{1}{2}$	-25 $\frac{1}{2}$	-34	-34	-36
23	7 AM.	-44	-42	-28	-29	-40	-40	-42
	4 PM.	-26	-24	-15	-16	-21	-21	-27
	8 PM.	-35	-33	-21	-22	-30	-30	-34
24	8 AM.	-42	-38	-26	-27	-36	-36	-40
	1 PM.	-18	-16	-7	-8	-15	-15	-22
	4 PM.	-12	-10	-3	-4	-8	-8	-14
	8 PM.	-14	-12	-3 $\frac{1}{2}$	-4	-10	-10	-15
25	8 AM.	-14	-12	-4	-5	-10	-10	-16
	Noon	-4	-2	+3 $\frac{1}{2}$	+4 $\frac{1}{2}$	-0	-0	-7
	4 PM.	-2	-0	+5	+4	+1	+1	-4
	8 PM.	-6	-4	+3	+2	-1	-1	-7
26	7 AM.	-22	-20	-11	-12	-18	-18	-22
	4 PM.	-10	-8	-1	-2	-6	-6	-12
	8 PM.	-18	-16	-7 $\frac{1}{2}$	-8	-14	-13	-17
27	8 AM.	-24	-22	-13	-14	-20	-20	-25
	Noon	-12	-10	-4	-5	-10	-10	-17
	4 PM.	-14	-12	-4	-5	-10	-10	-15
	8 PM.	-20	-18	-9 $\frac{1}{2}$	-10 $\frac{1}{2}$	-15	-15 $\frac{1}{2}$	-20

Thermometers

Thermometers compared.

Year, Month, and Day.	Hours.	A.	B.	D.	E.	F.	G.	H.
1782								
Feb. 28	8 AM.	-20	-18	-9	-10	-15	-15½	-22
	Noon	-8	-6	+1	-0	-4	-5	-11
	8 PM.	-12	-10	-6	-7	-10	-10	-12
Mar. 1	8 AM.	-6	-4	+3	+2	-2	-2	-8
	Noon	-6	-4	+3	-2	-2	-2	-7½
	4 PM.	-14	-12	-4	-5	-10	-10	-14
	8 PM.	-18	-16	-8	-9	-14	-14	-18
2	9 AM.	-20	-23	-13½	-14	-20	-21	-26
	4 PM.	-15	-12	-5	-6	-10	-10½	-16
	8 PM.	-22	-20	-11	-12	-17	-17	-22
3	8 AM.	-36	-34	-22	-23	-31	-31½	-35
	1 PM.	-12	-10	-2	-3	-7	-7	-16
	4 PM.	-10	-8	-2	-3	-6	-6	-12
	8 PM.	-18	-16	-7	-8	-13	-13½	-18
4	8 AM.	-24	-22	-12	-13	-19½	-20	-25
	Noon	-8	-6	-0	-1	-4½	-5	-7
	4 PM.	-12	-10	-3	-4	-7½	-8	-13
	8 PM.	-20	-18	-9	-10	-15	-15	-18
5	8 AM.	-32	-30	-19	-20	-26	-26	-30
	Noon	-16	-14	-6	-7	-12½	-13	-19
	4 PM.	-14	-12	-4	-5	-10	-10	-16
	8 PM.	-18	-16	-8	-9	-14	-14	-19
6	8 AM.	-19	-17	-8½	-9½	-15	-15	-20
	Noon	-10	-8	-1	-2	-6	-6	-13
	4 PM.	-12	-10	-2	-3	-7½	-8	-13
	8 PM.	-20	-18	-10	-11	-16	-16	-20
7	8 AM.	-19	-17	-8	-9	-15	-15	-21
	3 PM.	-3	-1½	+5	+4	-0	-0	-8
	4 PM.	-0	+2	+7	+6	+4	+4	-3
	8 PM.	-0	+2	+7	+6	+4	+4	-3
8	Noon	+26	+28	+29½	+28½	+29	+28	+21
	4 PM.	+16	+18	+17	+17	+17	+17	+14
	8 PM.	-0	+2	+7	+6	+4	+4	+4
9	8 AM.	-29	-26	-16	-17	-24	-24½	-28
	Noon	-14	-12	-4	-5	-10	-10	-16
	4 PM.	-11	-9	-2	-3	-6½	-7	-13
	8 PM.	-16	-14	-6	-7	-12	-12	-16

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Thermometers.



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**Thermometers compared.**

**Thermometers**

Thermometers compared.

Year, Month, and Day.	Hour.	A.	B.	D.	E.	F.	G.	H.	
1782									
Mar. 20	8 AM.	- 4	- 2	+ 4 $\frac{1}{2}$	+ 3 $\frac{1}{2}$	- 0	- 0	- 6	
	4 PM.	+ 6	+ 8	+ 11 $\frac{1}{2}$	+ 10 $\frac{1}{2}$	+ 9	+ 8 $\frac{1}{2}$	+ 2	
	8 PM.	+ 2	+ 4	+ 9	+ 8	+ 6	+ 6	- 0	
21	8 AM.	+ 3	+ 5	+ 10	+ 9	+ 7	+ 6	- 0	
	Noon	+ 14	+ 16	+ 18 $\frac{1}{2}$	+ 17 $\frac{1}{2}$	+ 17	+ 16	+ 9	
	4 PM.	+ 14	+ 16	+ 19	+ 18	+ 19	+ 18	+ 11	
	8 PM.	+ 12	+ 14	+ 17	+ 16	+ 15	+ 14 $\frac{1}{2}$	+ 9	
22	Noon	+ 12	+ 14	+ 17	+ 16	+ 16	+ 15	+ 8	
	4 PM.	+ 14	+ 16	+ 19	+ 18	+ 18	+ 17	+ 11	
	8 PM.	+ 10	+ 12	+ 16	+ 15	+ 14	+ 14	- 8	
23	8 AM.	- 6	- 4	+ 3	+ 2	- 1	- 1	- 7	
	Noon	+ 4	+ 6	+ 11	+ 10	+ 9	+ 8 $\frac{1}{2}$	- 0	
	8 PM.	- 0	+ 2	+ 7	+ 6	+ 4	+ 4	- 2	
24	8 AM.	- 0	+ 2	+ 8	+ 7	+ 5	+ 4	- 3	

Experiment

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Experiment I. made December 15, 1781.

Time per Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. / "				
9 22 0	25	23	—	Put them into a tumbler of snow.
9 31 0	—	—	—	Added the spirit of nitre.
9 32 0	60	40	—	Thermometer descends quick and equable.
9 32 20	88	40	—	
9 32 38	108	40	—	
9 32 55	120	40	—	
9 33 10	134	40	—	
9 33 29	150	40	—	
9 33 40	160	40	—	
9 34 0	176	40	—	
9 34 35	200	40	—	
9 34 57	214	40	—	
9 35 10	220	40	—	
9 35 50	252	40	—	
9 36 0	260	40	—	
9 36 29	280	40	—	
9 37 7	300	40	—	
9 37 48	320	40	—	
9 38 30	340	40	—	
9 39 6	352	40	—	
9 39 20	360	40	—	
9 39 48	370	40	—	
9 40 6	376	40	—	
9 40 38	384	40	—	
9 40 50	392	40	—	
9 41 19	400	40	—	
9 41 40	406	40	—	
9 42 10	414	40	—	
9 42 30	420	40	—	
9 43 0	426	40	—	
9 43 36	434	—	—	
9 44 0	438	40	—	
9 44 30	444	40	—	
9 45 0	448	40	—	
9 45 40	448	40	—	
9 54 0	448	40	—	

Experiment I. made December 15, 1781.

Time per Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. / "				
9 55 0	448	40	—	Made a second freezing mixture.
9 56 15	448	40	—	Removed the instruments into the second mixture.
9 56 40	448	40	—	
9 57 0	448	40	—	
9 58 0	448	40	—	
9 58 40	448½	40	—	
9 59 0	448½	40	—	
10 0 0	448½	40	—	
10 0 40	448½	40	—	Added more spirit of nitre to the freezing mixture.
10 2 0	448½	40	—	
10 3 35	448½	40	—	{ Took the apparatus out of the freezing mixture, found it frozen, and immediately re-placed it.
10 4 14	448½	42	—	
10 5 0	448	42	—	
10 7 10	448	41	—	
10 9 0	448	40	—	
10 9 40	448	40	—	
10 11 0	448	40	—	{ Took the apparatus out again, and endeavoured to withdraw the thermometer, but could not effect it, the quicksilver in the cylinder being frozen; put the apparatus again into the mixture.
10 11 34	448	40	—	
10 12 5	448	40	—	
10 12 20	448	40	—	
10 13 28	448	40	—	
10 15 0	—	—	—	Made a third freezing mixture.
10 16 15	444	40	—	
10 16 30	442	40	—	
10 17 10	440	40	—	
10 17 26	438	40	—	
10 17 40	—	—	—	Removed the instruments into the third mixture.
10 18 13	446	40	—	
10 18 40	448	40	—	
10 19 0	448	40	—	
10 20 0	448	40	—	Went away to warm myself.
10 26 0	448	43	—	Returned.
10 27 0	448	43	—	

Experiment

Experiment I. made December 15, 1781.

Time per Watch.		Thermom. below O.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h.	' "				
10	27 30	448	43	—	{ Put a spirit thermometer (D) into the freezing mixture along with apparatus and the mercurial thermometer.
10	28 0	448	43	14½	
10	28 30	448	43	25	
10	28 40	448	43	28	
10	28 50	448	43	30	
10	29 0	448	43	31	
10	29 40	448	43	31½	
10	30 0	447½	42½	31½	
10	30 50	447½	42½	31	
10	31 30	447½	42½	18	
10	31 50	447½	42	24	{ Took out the spirit thermometer (D). Put in another spirit thermometer (E).
10	32 0	447½	42	28	
10	32 20	447½	42	31	
10	32 40	447½	42	31½	
10	32 55	448	41½	32	
10	33 20	448	41½	32½	
10	33 50	448	41½	32½	
10	34 0	448	41	32½	
10	34 50	447½	40½	—	
10	35 20	447½	40½	—	{ Removed the Instruments back into the second mixture.
10	35 40	440	40	—	
10	35 49	433	40	—	
10	36 0	420	40	—	
10	36 20	410	40	—	
10	36 50	400	40	—	
10	37 8	392	40	—	
10	37 20	388	40	—	
10	37 40	380	40	—	
10	38 0	372	40	—	
10	38 20	366	40	—	
10	38 33	360	40	—	
10	40 0	324	40	—	
10	42 0	270	40	—	
10	43 0	260	40	—	
10	44 0	248	40	—	

Experiment

Experiment I. made December 15, 1781.

Time <i>per</i> Watch.	Thermom. below 0.	Appara- tus.	Spirit Thermom.	Remarks and Occurrences.
h. "				
10 44 15	244	40	—	Removed the instruments back to the 3d mixture.
10 44 35	246	40	—	
10 45 0	248	40	—	
10 46 0	247	40	—	
10 47 0	247	40	—	
10 48 0	246½	40	—	
10 49 0	246	40	—	
10 50 0	246	40	—	
10 51 0	245½	40	—	
10 52 0	245½	40	—	
10 53 0	244	40	—	
10 55 0	244	40	—	
10 55 20	244	40	—	
11 4 0	114	38	—	Went away, to warm myself. Returned.
11 9 0	54	37	—	Put in the spirit thermometer (D).
11 9 50	48	37	—	
11 11 0	40	36	—	
11 12 0	39	35½	—	
11 13 0	—	—	24	Took out the apparatus; the quicksilver was perfectly fluid, and the inclosed thermometer (F) was easily withdrawn.
11 13 40	38	35½	—	
11 14 0	38	—	26½	

*Remarks and observations on the first experiment.*

Finding the thermometer on Thursday evening, the 14th of December, was 18° below the cypher, I concluded the morning would afford me an opportunity to make an attempt to fix the point at which quicksilver begins to freeze; I therefore put a bottle of spirit. nitri fortis upon the top of the house in open air, that it might be of the same temperature when it was to be

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used.

used. The thermometer had been hung up before, three weeks, in the open air, to compare their scales. At 7 o'clock in the morning of the 15th, the thermometers were about  $23^{\circ}$  below nought; I therefore made preparations for the experiments, getting the quicksilver out into the air, providing glass tumblers for mixing the nitrous acid with the snow, &c. I put as much quicksilver into a glass cylinder as (when the thermometer (F) was introduced) just filled the bulbous part of the cylinder; the scale of the thermometer did not reach the length of the tube by about three inches; and the bare part of the tube was wound round with red worsted in two places, to a thickness sufficient to fill the upper part of the orifice of the cylinder in order to exclude the external air: now, as the quicksilver only filled the bulb, there was a space of near half an inch left empty between the quicksilver and the nearest piece of worsted, so that, by inclining the apparatus, the quicksilver readily ran out of the bulb into the other part of the cylinder. This was done with an intention to discover the more easily when the quicksilver ceased to be fluid; for, by taking the instrument out of the freezing mixture, and elevating the lower end, the quicksilver, if not frozen, would run into the void space.

The experiment was made in the open air, on the top of the Fort, with only a few deer-skins sewed together, placed to windward for a shelter: there was plenty of snow (eighteen inches deep) upon the works, and the thermometers were close at hand. In thrusting the thermometer (F) into the quicksilver, the instrument rose to the cypher, but soon began to descend again; but being unwilling to lose time, I stuck the apparatus into the snow, the sooner to bring it to the temperature of the air.

The

The table will fully explain the process. I was in hopes, by shifting the instruments into three fresh mixtures, I should have been able to have produced a greater degree of cold than by one only; yet it did not. I added more spirit of nitre, but without effect. At 10 h. 3' 35'' I took out the apparatus, and raised the bulbous end to make the quicksilver run, but found it was frozen, so that it did not alter its figure in the least. I then placed it in the mixture, where it continued till 10 h. 11', when I made another trial as before, but without perceiving any alteration: however, to be more certain of its being frozen, I proposed to take out the thermometer; but all the strength in my fingers could not move it in the least, so that myself and officers, who stood by, were convinced it was frozen fast. I then made another mixture in hopes to augment the cold, and make the inclosed thermometer (F) descend; however, seeing no alteration, I went into the house to warm myself, and on my return found it had fallen 3°. I tried the coldness of the mixture by different spirit thermometers, and afterwards shifted the instruments into the mixture from whence I had taken them; but this diminished the cold by the thermometer, so that I re-placed them again in the third mixture, and the quicksilver in the thermometer descended again to its former point 448°. I continued observing it some minutes, when the cold obliged me a second time to retire, and on my return found both the thermometer and apparatus rising: on dipping a spirit thermometer into the mixture, I found it had a considerable degree of coldness, and both the apparatus and mercurial thermometers were nearly equal. I then took them out, and the quicksilver in the cylinder was as fluid as when it was first poured in.

I should have observed, that during the time I was pouring in the spirit of nitre at the beginning of the operation, I was



so engaged in mixing it with the snow, that I did not see the thermometer in the apparatus sink to  $40^{\circ}$ , which must have been very sudden, because I was but one minute before I observed it. I could observe no alteration in the quicksilver in the cylinder when it was frozen, and intending to make more experiments, I was unwilling at this time to break the glass.

The time was taken by a good watch which shews seconds, and (which however I apprehend can be of little consequence) about  $5' 10''$  too fast by apparent time. I had two assistants; one to repeat audibly every second, and the other to write down the time and the observations as fast as I made them.

The observations were taken down with a pencil, but copied fair with ink into my note book: they were compared the same day the experiment had been made, to avoid mistakes; and these remarks were written at the same time, whilst the remembrance of them was yet strong on the mind.

The thermometers used on this occasion were those marked **A** and **F**.

Experiment II. made Dec. 16, 1781.

Time per Watch.	Thermom. below C.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. / ' / "				
8 19 30	34	31	—	Put the instruments into a tumbler of snow. Began to pour in the spirit. nitri fortis.
8 21 30	—	—	—	
8 21 45	40	34	—	
8 22 12	40	36	—	A large proportion of spirit of nitre poured in. Adding snow to the mixture, it being too thin.
8 22 40	40	36	—	
8 23 0	32	32	—	
8 23 31	38	29	—	
8 23 50	40	30	—	
8 24 0	43	34	—	
8 24 15	44	36	—	
8 24 40	58	40	—	
8 24 50	66	43	—	
8 25 0	76	43	—	
8 25 19	80	40	—	
8 25 29	84	40	—	
8 25 46	90	40	—	
8 26 0	94	40	—	
8 26 12	100	40	—	
8 26 24	104	40	—	
8 26 43	110	40	—	{ Added more snow, the quantity of the mixture being small.
8 27 0	116	40	—	
8 27 22	126	40	—	
8 27 42	138	40	—	
8 28 0	146	40	—	
8 28 29	158	40	—	{ Found the mixture did not cover the bulb of the mercurial thermometer. Poured in more spirit of nitre.
8 28 45	164	40	—	
8 29 0	168	40	—	
8 29 25	176	40	—	
8 29 52	180	40	—	
8 30 20	184	40	—	
8 30 50	210	40	—	
8 31 20	160	40	—	
8 31 28	156	40	—	
8 31 43	148	40	—	
8 32 0	152	40	—	Put in snow by degrees, and stirred the mixture. Ditto,

## Experiment II. made December 16, 1781.

Time <i>per</i> Watch.			Thermom. below 0.	Appara- tus.	Spirit Thermom.	Remarks and Occurrences.
h.	'	"				
8	32	10	156	40	—	
8	32	20	162	40	—	
8	32	31	168	40 $\frac{1}{2}$	—	
8	32	48	178	42 $\frac{1}{2}$	—	
8	33	0	188	43	—	
8	33	15	194	44	—	
8	33	26	200	44 $\frac{1}{2}$	—	
8	33	39	206	92	—	{ The mercury in the apparatus thermometer sunk so instantaneously, I could not catch any intermediate degrees.
8	33	47	206	95	—	
8	34	0	206	95	—	
8	34	21	206	95	—	
8	34	35	206	95	—	
8	35	0	206	95	—	
8	35	30	206	95	—	
8	36	0	206	95	—	
8	36	30	206	95	—	
8	37	0	206	95	30	
8	37	30	206	95	32	Put in the spirit thermometer D.
8	38	0	206	95	33	
8	38	30	206	95	33	
8	39	0	206	95	33	
8	40	0	206	95	33	
8	41	0	206	95	33	
8	42	0	206	95	33	
8	43	0	206	95	33	
8	44	0	206	95	33	
8	45	0	206	95	33	
8	46	0	206	95	33	{ Took out the apparatus, and found the quicksilver in the cylinder frozen; replaced it.
8	46	30	206	95	33	
8	46	54	206	95	33	{ From 8 h. 47' to 9 h. 11' employed in making the next experiment.
8	47	0	206	95	33	
8	48	0	206	95	33	

Experiment

Experiment II. made December 16, 1781.

Time per Watch.			Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h.	'	"				{ Went to breakfast, and, seeing no alteration, intended only to return now and then, it being Sunday, and prayer time being at hand.
9	11	0	206	95	32½	
9	50	0	206	95	31½	
10	35	0	378	Bulb	27	
10	37	10	348	D°	—	
10	38	0	320	D°	—	{ Took the apparatus out to examine it; then put it in again.
10	38	40	310	D°	—	
10	39	30	306	F°	—	
10	40	30	296	D°	—	
10	41	20	290	D°	27	
10	41	50	284	D°	—	
10	42	15	280	D°	27	
10	43	0	273	D°	—	
10	43	50	260	D°	—	
10	44	50	250	D°	—	
10	46	10	234	D°	—	
10	46	40	227	D°	—	
10	47	0	222	D°	26	
10	47	30	216	D°	26	
10	48	15	206	D°	—	
10	48	35	202	D°	—	
10	49	10	194	D°	—	
10	49	30	189	D°	26	
10	50	0	182	D°	—	
10	50	20	177	D°	—	
10	50	50	170	D°	26	
10	51	15	164	D°	—	Stirred the instruments about in the mixture.
10	51	40	156	D°	26	
10	52	20	147	D°	—	
10	52	50	141	D°	—	
10	55	5	112	D°	25½	
10	55	30	107	D°	—	
10	56	0	102	D°	—	
10	56	15	98	D°	25	
10	56	35	94	D°	—	
10	56	50	91	D°	—	

Experiment

Experiment II. made December 16, 1781.

Time per Watch.		Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h.	"				
10	57 10	86	Bulb	—	
10	57 30	82	D°	—	
10	57 45	79	D°	25	
10	58 5	75	D°	—	
10	58 30	70	D°	—	
10	58 45	67	D°	—	
10	59 10	62	D°	—	
10	59 30	58	D°	—	
11	0 0	53	D°	—	
11	0 15	50	D°	—	
11	0 40	47	D°	24½	
11	1 0	44	D°	24½	
11	1 20	41	D°	—	
11	2 0	40	D°	—	
11	2 30	39	D°	—	
11	3 0	39	D°	—	
11	3 30	39	D°	—	
11	4 0	38½	D°	—	{ The mercury in the apparatus thermometer raising up the tube from the bulb.
11	4 20	38	235	—	
11	4 30	38	225	—	
11	4 40	38	220	—	
11	4 50	38	218	—	
11	5 0	38	205	—	
11	5 10	38	195	—	
11	5 20	38	183	—	
11	5 33	38	172	—	
11	5 44	38	163	—	
11	5 55	38	154	—	
11	6 10	38	140	—	
11	6 20	38	130	—	
11	6 30	37	120	—	
11	6 40	37	110	—	
11	6 50	37	97	—	
11	7 0	37	87	—	
11	7 10	37	78	—	
11	7 20	37	67	—	

Experiment

Experiment II. made December 16, 1782.

This experiment was made with the same instruments as the preceding, and the quicksilver which was left yesterday in the cylinder was the same now employed. I was rather unfortunate in making too small a quantity of the freezing mixture at the beginning, which obliged me to make repeated additions to it: by this means the operation was not only retarded, but sometimes it even undid what had been done; for in pouring in the nitrous acid it was unavoidable but part of it should come in contact with the bulbs of the instruments before it was mixed with snow. In this case it never failed making the thermometers rise suddenly much higher than where they stood before the spirit was added; and at length it only descended to  $206^{\circ}$ , which is not half so low as on the preceding day, though the temperature of the air was ten degrees colder (*viz.*  $34^{\circ}$ ): yet it is remarkable, that though the thermometer was so much higher, the apparatus was sunk more than twice as low as the day before; for after having been long stationary at  $40^{\circ}$ , it sunk

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to 95°. I then made a fresh mixture, but it had no effect any way during three quarters of an hour I attended to it afterwards. During this idle interval I made the third experiment. Finding no alteration, I went down to breakfast, and on my return was surpris'd to find the quicksilver in the apparatus thermometer had subsided into the bulb, and the standard thermometer had been very low (how low I cannot tell), and was rising briskly. The spirit thermometer also shewed the mixture had a less degree of cold than before. To be certain that the quicksilver in the apparatus thermometer was in the bulb, I took the apparatus out of the mixture, and examined it minutely for half a minute, till I was quite certain of it; and also that the quicksilver in the cylinder was frozen, and it is remarkable, it did not liquify in all that time.

The observations were made with the greatest attention, and (on every particular occasion) noted down as quick as possible.

Experiment

Experiment III. made December 16, 1781.

Time per Watch.			Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h.	'	"	B.	G.	E.	
8	55	55	34	35	—	{ Put them into the first freezing mixture used in the preceding experiment.
8	56	10	40	40	—	
8	56	21	41	41	—	
8	56	42	42	42	—	
8	57	0	43	42½	—	
8	57	26	43	43	—	
8	58	0	43	43	—	
8	58	58	54	43	—	
8	59	14	60	43	—	
8	59	40	64	43	—	
9	0	0	68	43	—	
9	0	34	70	43	—	
9	1	0	75	43	—	
9	1	22	78	43	—	
9	2	0	82	43	—	
9	2	30	85	43	—	
9	3	0	88	43	—	
9	4	0	94	43	—	{ Put the spirit thermometer (E) into the mixture.
9	4	21	94	43	27	
9	4	40	98	43	30	
9	5	12	100	43	31½	
9	6	0	104	43	32	
9	6	30	106	43	32	
9	7	0	108	43	32	
9	7	40	110	43	32	
9	8	20	112	43	—	
9	9	0	114	43	—	
9	10	30	116	40	—	{ The quicksilver in the apparatus was fluid, but seemed thick and in grains, somewhat like crumbs of bread; replaced it again, and went to breakfast. Examined the quicksilver again, it was frozen hard. The quicksilver as fluid as ever.
9	50	0	40	40	—	
10	55	40	—	37½	—	

This experiment was made during the continuance of that which immediately precedes it, as may be seen by examining

\*X x 2

the



the time by the watch, and was the effect of chance; for the first freezing mixture, which had been used in the second experiment, standing in the glass close to me (and the other instruments being long stationary, did not require particular attention), I took down the thermometer (G) and charged its cylinder with quicksilver, as in the other examples, and suspended it in the old mixture, together with the mercurial thermometer (B) and a spirit thermometer; the mixture seemed to have lost much of its coldness, as appeared by the thermometers. It seemed very extraordinary to me, that the apparatus, after having been so long stationary at  $43^{\circ}$ , should yet contain fluid quicksilver; but both myself and assistant thought it was thicker than ordinary, as it did not run freely, but seemingly in pieces (not globules): however we put it back again into the mixture, and set it by as of no further use; but returning after breakfast, we found it was firmly frozen, so as to give no appearance of fluidity though the included thermometer was only at  $40^{\circ}$ , which I look upon to be the exact freezing point of quicksilver; and then the congelation was in fact begun before, and effected by only a longer continuance in the same degree of cold.

It may be necessary to mention, that the space between the bottom of the ivory scale to the bulb of the thermometer (F) which made part of the apparatus used in the second experiment, was two inches nine-tenths; and when taken with a pair of compasses (dividers) with one foot placed at the cypher 0 on the graduated scale, the other extended to  $148^{\circ}$  if measured upwards, and to  $165^{\circ}$  if measured downwards, for the divisions were unequal.

Experiment

Experiment IV. made January 7, 1782.

Time per watch.			Thermom. below O.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h.	'	"				
8	7	0	35	30	—	Put the instruments into a tumbler of snow.
8	9	45	35½	27	—	
8	10	40	—	—	—	Added the spirit of nitre.
8	11	0	42	42	—	{ Added more snow to increase the quantity of the mixture.
8	12	30	—	—	—	Added more spirit.
8	13	15	68	42	—	
8	13	35	78	42	—	
8	14	0	92	42	—	
8	14	15	100	42	—	
8	14	30	108	42	—	
8	14	45	116	42	—	
8	15	0	126	42	—	
8	15	15	134	42	—	
8	15	30	140	42	—	
8	15	45	146	41½	—	
8	16	20	160	41½	—	
8	16	30	166	41½	—	
8	16	45	171	41½	—	
8	17	15	182	41½	—	
8	17	45	191	41½	—	
8	18	0	196	41½	—	
8	18	15	202	41½	—	
8	18	30	208	41½	—	
8	18	45	212	41½	—	
8	19	30	232	77	—	The descent in the apparatus therm. very quick.
8	19	40	240	77	—	
8	20	30	258	77½	—	
8	20	40	262	77½	—	
8	21	0	270	77½	—	
8	21	15	278	77½	—	
8	21	45	280	77½	—	
8	22	0	286	77½	—	
8	22	30	293	77½	—	
8	22	35	298	77½	—	
8	23	0	302	78	—	
8	23	15	305	78	—	

Experiment IV. made January 7, 1782.

Time per Watch.		Thermom. below 0.	Appara- tus.	Spiri Thermom.	Remarks and Occurrences.
h.	m.				
8	23	30	309	78	
8	24	0	316	78	
8	24	15	319	78	
8	24	30	322	78	
8	24	45	327	78	
8	25	0	329	78	
8	25	15	332	78	
8	25	30	336	78	
8	25	45	338	78	
8	26	0	342	78	
8	26	15	345	78	
8	26	30	348	78	
8	26	45	351	78	
8	27	0	354	78	
8	27	15	356	78	
8	27	30	359	78	
8	27	45	361	78	
8	28	0	364	78	
8	28	15	366	78	
8	28	30	368	78	
8	28	45	370	78	
8	29	0	373	77	
8	29	15	376	77	
8	29	30	378	77	
8	29	45	380	77	Stirred the mixture.
8	30	15	386	77	
8	30	30	388	77	
8	31	0	392	77	
8	31	15	394	76½	
8	31	30	396	76½	
8	31	45	398	76½	
8	32	15	402	76½	
8	32	30	403	76	
8	32	45	404	76	
8	33	0	406	76	
8	33	15	408	76	Put in spirit thermometer (D).
8	34	0	412	76	31½

Experiment IV. made January 7, 1782.

Time per Watch.			Thermom. below 0.	Appara- tus.	Spirit Thermom.	Remarks and Occurrences.
h.	'	"				
8	34	30	416	76	32	
8	35	0	418	76	32	
8	35	30	422	76	32	
8	36	0	424	76	32	
8	36	15	425	76	32	
8	36	30	425	76	32	
8	36	45	427	76	32	
8	37	0	428	76	32	
8	37	15	429	76	32	
8	37	30	430	76	32	
8	37	45	430	76	32	
8	38	0	431	76	32	
8	38	30	432	76	32	
8	38	45	433	76	32	
8	39	0	434	76	32	
8	39	15	435	76	32	
8	39	30	435½	76	32	
8	40	0	436	76	32	
8	40	30	436	76	32	
8	40	45	437	76	32	
8	41	15	438	76	32	
8	41	30	438½	76	32	
8	41	45	439	76	31½	
8	42	15	440	76	31½	
8	43	0	440	76	31½	
8	43	15	440	75½	31½	
8	44	30	440	75½	31	Made a fresh mixtur.
8	47	0	438	75	31	Removed the instr. into the new freezing mixture.
8	47	36	448	76	34½	
8	48	0	448	76½	35	
8	48	30	448	77	35	
8	49	0	448	77	35	
8	49	30	449	77	35	
8	50	0	449	77	35	
8	50	30	450	77	35	
8	51	0	450	77	35	
8	52	0	450	77	35	

Experiment

Experiment IV. made January 7, 1782.

Time <i>per</i> Watch.	Thermom. below 0	Appara- tus.	Spirit Thermom.	Remarks and Occurrences.
h.				
8 53	0 45°	77	35	
8 54	0 45°	77	35	
8 56	0 45°	77	35	
8 57	0 45°	77	35	
8 58	0 45°	77	35	
8 59	0 45°	77	35	
9 0	0 45°	77	35	
9 1	0 45°	77	35	
9 2	0 45°	77	35	
9 3	0 45°	77	35	
9 4	0 45°	77	35	
9 5	0 45°	77	35	
9 6	0 45°	77	35	
9 7	0 45°	77	35	Examined the apparatus; found all solid.
9 8	0 45°	77	35	
9 9	0 45°	77	35	
9 10	0 45°	77	35	
9 11	0 45°	77	35	
9 12	0 45°	77	35	
9 13	0 45°	77	35	
9 14	0 45°	77	35	
9 15	0 45°	77	35	
9 16	0 45°	77	35	
9 17	0 45°	77	35	
9 18	0 45°	77	35	
9 19	0 45°	77	35	
9 20	0 45°	77	35	
9 21	0 45°	77	35	
9 22	0 45°	77	35	
9 23	0 45°	77	35	Added more snow.
9 23 30	0 45°	77	35	
9 24	0 45°	77	34½	
9 25	0 45°	77	34½	
9 26	0 45°	77	34½	
9 27	0 45°	77	34½	
9 28	0 45°	77	34½	

Experiment

Experiment IV. made January 7, 1782.

Time per Watch.		Thermom. below 0.	Appara- tus.	Spirit Thermom.	Remarks and Occurrences.
h.	"				
9	29	0 450	77	34 $\frac{1}{2}$	
9	30	0 450	77	34 $\frac{1}{2}$	
9	31	0 450	77	34 $\frac{1}{2}$	
9	32	0 450	77	34 $\frac{1}{2}$	
9	33	0 450	77	34 $\frac{1}{2}$	
9	34	0 450	77	34 $\frac{1}{2}$	
9	35	0 450	77	34 $\frac{1}{2}$	
9	36	0 450	77	34 $\frac{1}{2}$	
9	37	0 449	77	34	
9	38	0 449	77	33 $\frac{1}{2}$	
9	39	0 449	77	33	
9	40	0 449	77	33	
9	41	0 449	77	32 $\frac{1}{2}$	
9	42	0 449	77	32 $\frac{1}{2}$	
9	43	0 448 $\frac{1}{2}$	76	32 $\frac{1}{2}$	
9	44	0 448 $\frac{1}{2}$	76	32 $\frac{1}{2}$	
9	45	0 448 $\frac{1}{2}$	76	32 $\frac{1}{2}$	
9	46	0 448 $\frac{1}{2}$	76	32	
9	47	0 448 $\frac{1}{2}$	76	32	
9	48	0 448 $\frac{1}{2}$	76	32	
9	49	0 448 $\frac{1}{2}$	76	32	
9	50	0 448 $\frac{1}{2}$	76	31 $\frac{1}{2}$	
9	51	0 448 $\frac{1}{2}$	76	31 $\frac{1}{2}$	
9	52	0 448	75 $\frac{1}{2}$	31 $\frac{1}{2}$	
9	53	0 448	75 $\frac{1}{2}$	31 $\frac{1}{2}$	
9	54	0 448	75	31 $\frac{1}{4}$	
9	56	0 448	75	31	
9	57	0 447	75	31	
9	58	0 445	75	30 $\frac{3}{4}$	
9	58	30 444	75	30 $\frac{3}{4}$	
9	58	45 443	75	30 $\frac{3}{4}$	
9	59	0 442	75	30 $\frac{3}{4}$	
9	59	15 441	75	30 $\frac{3}{4}$	
9	59	30 440	75	30 $\frac{1}{2}$	
9	59	45 439	75	30 $\frac{1}{2}$	
10	0	0 438	75	30 $\frac{1}{2}$	
10	0	15 437	75	30 $\frac{1}{2}$	

## Experiment IV. made January 7, 1782.

Time per Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and observations.
h.				
10 0 30	436	75	30½	
10 0 45	435	75	30½	
10 1 0	434	75	30½	
10 1 15	433	75	30½	
10 1 30	432	75	30½	
10 1 45	431	75	30½	
10 2 0	430	75	30	
10 2 15	429	75	30	
10 2 45	426	74	30	
10 3 30	422	74	30	
10 4 15	417	74	30	
10 4 45	414	74	30	
10 5 0	412	74	30	
10 5 30	408	73½	30	
10 6 0	405	73½	30	
10 6 30	401	73½	29½	Filled up the tumbler with a former mixture.
10 7 30	388	73	28½	
10 8 0	382	74	28	
10 8 30	377	82	28	
10 8 46	374	87	28	
10 9 0	368	110	28	
10 9 10	366	125	28	
10 9 20	364	170	28	
10 9 40	360	200	28	
10 9 50	358	240	28	
10 10 0	356	Bulb	28	
10 10 30	350	D°	28	
10 11 10	340	D°	28	
10 12 0	332	D°	28	
10 12 30	327	D°	28	
10 13 0	321	D°	28	
10 13 30	316	D°	28	
10 14 0	311	D°	28	
10 14 30	306	D°	28	
10 15 0	301	D°	28	
10 15 30	296	D°	28	Made a fresh mixture.
10 16 0	—	D°	28	Put the instruments into a fresh mixture.

Experiment

**Experiment IV. made January 7, 1782.**

Time per Watch.		Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h.	"				
16	30	312	Bulb	29	
17	0	352	D°	35	
17	45	373	D°	35	
18	0	384	D°	35	
18	15	389	D°	35	
18	30	389	D°	35	
18	45	389	D°	35	
19	0	389	D°	35	
20	0	389	D°	35	
20	30	389	D°	35	
21	0	389	D°	35	
22	0	389	D°	35	
23	0	389	D°	35	
24	0	389	D°	35	
25	0	389	D°	35	
26	0	389	D°	35	
27	0	389	D°	35	
28	0	389	D°	35	
29	0	389	D°	35	
30	0	389	D°	35	
31	0	389	D°	35	
32	0	389	D°	35	
33	0	389	D°	35	
34	0	389	D°	35	
35	0	389	D°	35	
36	0	389	D°	35	
37	0	389	D°	35	
38	0	389	D°	35	
39	0	389	D°	35	
40	0	389	D°	35	
41	0	389	D°	35	
42	0	389	D°	35	
43	0	389	D°	35	
44	0	389	D°	34½	
45	0	389	D°	34½	
46	0	389	D°	34½	



## Experiment IV. made January 7, 1782.

Time per Watch.		Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h.	"				
0	47	0	389	Bulb	34½
0	48	0	389	D°	34½
0	49	0	389	D°	34½
0	50	0	389	D°	34
0	51	0	389	D°	34
0	52	0	389	D°	34
0	53	0	288	D°	34
0	55	0	288	D°	33½
0	59	0	288	D°	33½
I	I	0	288	D°	33
I	5	0	388	D°	32½
I	10	0	388	D°	32
I	15	0	444	D°	31½
I	15	10	446	D°	31
I	15	30	445	D°	31
I	15	45	444	D°	31
I	16	15	443	D°	31
I	17	0	441	D°	31
I	18	0	437	D°	31
I	19	0	434	D°	30
I	20	0	428	D°	30
I	21	30	418	D°	29½ { Whilst examining the apparatus, part of the quicksilver turned fluid.
I	22	30	412	D°	29½
I	23	0	408	D°	29½
I	23	30	404	D°	29½
I	24	0	400	D°	29
I	24	30	396	D°	29
I	25	30	390	D°	29 { The center of the quicksilver appeared globular and solid.
I	26	30	379	D°	29
I	28	0	365	D°	29
I	29	0	355	D°	29
I	29	30	352	D°	29
I	29	45	349	D°	29
I	30	0	346	D°	29

Experiment

**Experiment IV. made January 7, 1782.**

Time per Watch.		Thermom. below 0.	Appara- tus.	Spirit Thermom.	Remarks and Occurrences.
h.	"				
11	30 15	344	Bulb	29	
11	30 30	342	D°	29	
11	30 45	339	D°	29	
11	31 0	336	D°	29	
11	31 15	333	D°	29	
11	31 30	330	D°	29	
11	31 45	328	D°	29	
11	32 0	325	D°	29	
11	32 30	320	D°	29	
11	32 45	318	D°	29	
11	33 0	314	D°	29	
11	33 15	312	D°	29	
11	33 30	309	D°	28½	
11	33 45	306	D°	28½	
11	34 0	303	D°	28½	
11	34 15	301	D°	28½	
11	34 30	298	D°	28½	
11	34 45	295	D°	28½	
11	35 0	292	D°	28½	
11	35 15	289	D°	28½	
11	35 30	286	D°	28½	
11	35 45	283	D°	28½	
11	36 0	280	D°	28½	
11	36 15	277	D°	28½	
11	36 30	274	D°	28½	
11	37 0	267	D°	28½	
11	37 15	264	D°	28½	
11	37 30	262	D°	28½	
11	37 45	258	D°	28½	
11	38 0	255	D°	28	
11	38 15	252	D°	28	
11	38 30	249	D°	28	
11	38 45	246	D°	28	
11	39 0	241	D°	28	
11	39 30	237	D°	28	
11	39 45	234	D°	28	

**Experiment**

Experiment IV. made January 7, 1782.

Time per Watch.		Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h.	m.				
11	40	0	231	D°	28
11	40	30	224	D°	28
11	41	0	216	D°	28
11	41	30	211	D°	28
11	42	0	204	D°	27½
11	42	30	197	D°	27½
11	43	0	190	D°	27½
11	43	30	185	D°	27½
11	44	0	178	D°	27½
11	44	30	170	D°	27
11	45	0	160	D°	27
11	45	30	150	D°	27
11	46	0	142	D°	27
11	46	30	134	D°	27
11	47	0	125	D°	27
11	47	30	118	D°	27
11	48	0	109	D°	27
11	48	30	103	D°	27
11	49	0	96	—	27
11	49	15	90	227	27
11	49	30	88	215	27
11	49	40	86	205	27
11	49	50	84	193	27
11	50	0	82	180	27
11	50	10	80	170	27
11	50	20	78	157	27
11	50	30	73	145	27
11	50	40	73	132	27
11	50	50	72	120	27
11	51	0	69	105	27
11	51	10	68	88	27
11	51	20	64	65	27
11	51	35	62	45	27
11	51	50	59	37½	27
11	52	0	56	38	27
{ The quicksilver in the apparatus thermometer rising fast in the tube.					

Experiment

Experiment IV. made January 7, 1782.

Time per Watch.	Thermom. below O.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. / "				
11 52 30	53	38	27	Found the quicksilver wholly thawed in the cylinder.
11 53 0	46	38	27	
11 53 30	42	37	27	
11 54 0	40	37	26½	
11 54 30	39	37	26½	
11 55 0	39	37	26½	
11 55 30	39	37	26½	
11 56 0	39	37	26½	
11 56 30	38	36	26	

This experiment was made with the mercurial thermometer (A) and the apparatus (F) as in the first and second experiments. The day was clear, with little wind at W. by S. or W.S.W. which I have observed to be generally the case in this country in the coldest weather. The thermometers at 8 o'clock were as follows, according to the rotation of the letters from (A) to (G),  $39^{\circ}\frac{1}{2}$ ,  $36^{\circ}\frac{1}{2}$ ,  $35^{\circ}$ ,  $25^{\circ}$ ,  $25^{\circ}$ ,  $34^{\circ}\frac{1}{2}$ ,  $34^{\circ}$  below the cypher. The apparatus thermometer (F), after standing at  $42^{\circ}$  and  $41^{\circ}\frac{1}{2}$  for a considerable time, sunk at once to  $77^{\circ}$ , not gradually or by jumps, but suddenly, as a weight falleth. The great descent of the quicksilver in the index thermometer (A) to  $440^{\circ}$  in the first freezing mixture I impute to the coldness of the weather, but was surpris'd to find it did not sink more than  $10^{\circ}$  lower in the second mixture; and in the third it did not reach so low as in the preceding, which, indeed, might be accounted for by the air growing warmer as the sun approached the meridian. At 10 h.  $6^{\circ}\frac{1}{2}$  I poured some of the first mixture into the tumbler where the instruments were immersed in the second, but found it weakened it; I therefore mixed a fresh one at 10 h. 16'. It is however remarkable, that after pouring in the first mixture

on the second, the apparatus, which had risen a little before, sunk suddenly into the bulb. I have marked its progress as fast as I could catch it. Another extraordinary circumstance in this experiment is, that the mercurial thermometer (A) should not subside lower in the third than in the second mixture; whereas the spirit thermometer shewed an equal degree of cold, while the quicksilver in the apparatus thermometer was in the bulb. At 11 h. 21' I took the apparatus out to examine it, and, by shaking it in my hand, all of a sudden some of the quicksilver in the cylinder liquified; the concussion perhaps dissolved its solidity, for it was not above a minute out of the mixture. Wondering much at this unexpected phenomenon, as the quicksilver in the thermometer did not rise, I put it into the mixture again immediately; but finding the inclosed thermometer shewed no alteration, my curiosity determined me to examine it again; therefore, about four minutes after, I took it out a second time, and found the surface of the quicksilver in the cylinder was liquified about one-eighth of the whole quantity, as near as I could guess; the rest formed a solid ball, including the bulb of the thermometer, which easily accounts for the quicksilver in that instrument remaining stationary. Wishing to observe the whole process, and the cold being too severe for the same persons to stand in the open air for so long a time, I desired one of my officers, with an assistant, to mark down the observations at the times I went to warm myself, but by no means to make any alterations in my absence; by this means the observations were continued regularly for near four hours.

At the end of the experiments the thermometers (B), (C), (D), and (G), stood as follows,  $18^{\circ}\frac{1}{2}$ ,  $15^{\circ}$ ,  $9^{\circ}\frac{1}{2}$ ,  $15$ , which shews the alteration in the temperature of the air from the beginning. The thermometers (A), (D), and (F), were used in the experiment.

Experiment

Experiment V. made February 22, 1781.

Time per Watch.		Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.	
h.	m.	A.	G.	C.		
8	4	78	40	29½	Making the freezing mixture. Put the instruments into the mixture.	
8	9	89	40	29		
8	11	—	—	—		
8	11	70	40	31½	Added more snow.	
8	11	60	39½	32		
8	11	54	39½	32		
8	12	—	—	—	Added more snow.	
8	13	50	41	33½		
8	13	51	42	34		
8	14	51	72	34	Added more snow.	
8	14	51	78	34		
8	14	51	78	34		
8	15	—	—	—	Added more snow.	
8	15	52	78	35		
8	15	—	—	—		
8	16	52	79	35	The quicksilver in the cylinder fluid.	
8	17	52	79	35		
8	20	52	79	35		
8	25	52	79	35	Made a fresh mixture. { Removed the instruments into the new mixture.	
8	30	52	79	35		
8	30	52	79	35		
8	31	52	79	35	The quicksilver in the cylinder still fluid. Put in mercurial thermometer (B).	
8	32	52	79	35		64
8	32	52	79	35		80
8	33	52	79	35	The quicksilver in the cylinder solid frozen.	90
8	34	52	79	35		90
8	35	52	79	35		91
8	37	52	79	35	Took out all the instruments.	91
11	31	52	79	35		91

*Remarks on the fifth experiment.*

I had not intended to make any more experiments of this kind, thinking those already made had fully determined the freezing

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ing point of quicksilver; but the arrival of a gentleman, who wished to see it, induced me to repeat it again. The weather was clear and serene, the wind about S.S.W., and the several thermometers stood as follows, A  $82^{\circ}$ , B  $66^{\circ}$ , D  $34^{\circ}$ , E  $34^{\circ}$ , F  $42^{\circ}$ , G  $42^{\circ}$ , H  $46^{\circ}$ , at seven o'clock in the morning; and at eight o'clock they were A  $78^{\circ}$ , B  $114^{\circ}$ , D  $29^{\circ}\frac{1}{2}$ , E  $29^{\circ}\frac{1}{2}$ , F  $29^{\circ}\frac{1}{2}$ , G  $40^{\circ}$ , H  $43^{\circ}$ ; yet it is remarkable, that quicksilver which was constantly exposed to the air in a saucer was not froze. I impute the small descent of the quicksilver in the thermometers to the great degree of the cold in the atmosphere as in the sixth experiment, for there the effect was similar. The most remarkable circumstance in this day's operation was the sudden descent of the quicksilver in the apparatus thermometer, and the length of time it continued at  $79^{\circ}$  before the quicksilver in the cylinder became solid. The freezing mixture retaining an equal degree of cold for so long a time (as appeared by the spirit thermometer), and the consequent stationary situation of all the instruments, I apprehend, proceeded from the continual cold in the circum-ambient air; for at one o'clock the thermometers were risen but very little, being as follows, A  $35^{\circ}$ , B  $32^{\circ}$ , D  $21^{\circ}$ , E  $22^{\circ}$ , F  $30^{\circ}$ , G  $30^{\circ}$ , H  $34^{\circ}$ , the wind blowing brisk from N.W.

Experiment

Experiment VI. made January 11, 1782.

Time per Watch.	Standard spirit H.	Mercurial therm. C.	Spirit therm. D.	Remarks and Occurrences.
h. ' "				
8 15 0	42	50	27½	The instruments just put into the freezing mixture.
8 15 50	43	70	33	
8 16 15	43	100	34	
8 16 45	44	120	34	
8 17 15	44	140	34	
8 17 30	44	150	34	
8 17 45	44	155	34	
8 18 30	45	170	34	The mixture did not cover the bulb of (H). Added more snow and spirit.
8 19 0	—	—	—	
8 20 20	44	170	34	
8 21 0	45	170	34	
8 22 15	46	170	33	
8 22 50	46	170	33½	
8 23 30	46	170	33½	
8 24 30	46	170	33½	
8 25 10	46	171	33½	
8 26 0	46	171	33½	
8 27 10	46	172	33½	
8 27 45	46	172	33½	
8 29 30	46	172	33½	Added more snow.
8 30 30	46	172	33½	
8 35 0	46	172	33½	Made a fresh mixture. Put the instruments into a fresh mixture.
8 36 0	46	173	34½	
8 36 30	46	173	34½	
8 37 0	46	173	34½	
8 37 30	46½	173	34½	
8 38 30	47	174	35	
8 39 40	47	174	35	
8 40 30	47½	174	35	
8 41 15	48	174	35	
8 42 0	48	175	35	
8 43 0	48	175	35	
8 44 0	48	175	35	
8 45 0	48	175	35	
8 45 15	47½	175	35	
8 46 0	47½	175	35	
8 46 30	47½	175	35	

\*Z z 2

Experiment



Experiment VI, made January 11, 1782,

Time per Watch.	Standard spirit H.	Mercurial therm. C.	Spirit therm. C.	Mercurial th. A and B.	Remarks and Occurrences.	Mer. ther. A in air.
h. / "				A.		
8 47 30	47½	174	34½			
8 48 0	47½	174	34½	45	Put mercurial th. (A) into the mixture.	
8 50 0	47½	174	35	76		
8 50 30	47½	174	35	114		
8 50 45	47½	174	35	124		
8 51 0	47½	174	35	146		
8 51 30	47½	174	35	147		
8 52 30	47½	174	35	147		
8 53 30	47½	174	35	147½		
8 54 0	47½	174	35	147½		
8 55 0	47½	174	35	147½		
8 56 0	47½	174	35	147½		
8 56 30	47½	174	35	147½		
8 57 35	47½	175	35	148		
8 58 0	48	175	35	148		
8 59 0	48	175	35	148		
9 0 0	48	175	35	148		
9 1 0	48	175	35	148		
9 2 0	48	175	35	148		
9 3 0	48	175	35	148		
9 4 0	48	175	35	148		
9 5 0	48	175	35	148	Took out thermometer (A).	148
				B.		
9 8 0	48	174	35	70	Put in mercurial thermometer (B).	148
9 9 45	48	174	35	84		148
9 10 15	48	174	35	86		148
9 11 0	48	174	35	86	Added more snow.	148
9 13 0	48	173	35	86		148
9 15 0	47	173	34½	86		148
9 16 0	47	173	34½	86		148
9 17 0	46½	173	34½	86		148
9 18 0	46½	173	34½	86		148
9 19 30	47	174	34	86		148
9 20 30	47	174	34	86		147
9 22 30	47	174	34	86		147
9 23 30	47	173	34	86		146
9 24 30	47	173	34½	86		145½

Experiment VI. made January 11, 1782..

Time per watch.	Standard spirit H.	Mercurial therm. C.	Spirit therm. D.	Mercurial therm. B.	Remarks and Occurrences.	Mer. ther. A in air.
h.						
9 25 30	47	173	34½	86	Sunk instantaneously.	438
9 26 30	47	173	34½	86		436
9 27 45	47	173	34½	86		436
9 28 30	46	174	34½	86		435
9 29 15	46	173	34½	86		433
9 30 0	46	173	34½	86		432
9 31 0	46	172	34½	86		432
9 31 45	46	172	34½	86		430
9 33 40	47	174	34½	86		430
9 34 30	47	174	34½	86	{ I suspect this variation to have been occasioned by different persons reading off the numbers. }	430
9 35 30	47	173	34½	86		428
9 36 0	47	173	34½	86		426
9 37 0	47	173	34½	86		426
9 38 0	47	173	34½	86		426
9 39 0	47	173	34½	86		425
9 40 0	47	173	34½	86		424
9 41 0	47	173	34½	86		424
9 42 0	47	173	34½	86		423
9 43 0	47	173	34½	86		422
9 45 0	47	172	34	86		420
9 48 0	47	172	34	86	Put in apparatus (F) at -40°.	412
9 49 0	47	172	34	86		412
9 50 0	47	172	34	86	Took out apparatus (F) solid -40°.	412
9 51 0	47	172	34	86	Put in apparatus (G) fell directly to -210°.	410
2 53 0	47	172	34	86	Took out, ditto, solid, at 210°.	405
9 55 0	47	172	34	86		402
9 56 0	47	172	34	86		401
9 57 0	47	172	34½	86		400
9 58 0	47	172	34½	86		398
9 59 0	47	172	34½	86		396
10 0 0	47	172	34½	86		394
10 1 0	47	172	34½	86		392
10 2 0	47	172	34½	86		390
10 3 0	47	172	34½	86		388
10 4 0	47	172	34½	86		386

Experiment:

Experiment VI. made January 11, 1782.

Time <i>per</i> Watch.		Standard spirit H.	Mercurial therm. C.	Spirit therm. D.	Mercurial therm. B.	Remarks and Occurrences.	Mercurial B id air.
h.	"						
10	5	0	47	172	34	86	384
10	6	0	47	172	34	86	382
10	9	0	47	172	34	86	372
10	10	0	47	172	34	86	370
10	11	0	47	172	34	86	368
10	13	0	47	172	34	86	362
10	14	0	47	172	34	86	360
10	15	0	47	172	34	86	358
10	16	0	47	172	34	86	356
10	17	0	47	172	34	86	353
10	18	0	47	172	34	86	351
10	19	0	47	172	34	86	348
10	20	0	47	172	33½	86	346
10	21	0	47	172	33½	86	342
10	22	0	47	172	33½	86	340
10	23	0	47	172	33½	86	338
10	24	0	47	171	33½	86	334
10	25	0	47	171	33½	86	332
10	26	0	47	171	33½	86	330
10	27	0	47	171	34	86	328
10	28	0	47	171	34	86	324
10	29	0	47	171	34	86	320
10	30	0	47	171	34	86	316
10	31	0	47	171	34	86	314
10	32	0	47	171	34	86	300
10	32	30	47	171	34	86	298
10	33	0	47	171	34	86	280
10	33	30	47	171	34	86	274
10	34	0	46	171	34	86	272
10	35	0	46	171	34	86	268
10	36	0	46	171	34	86	264
10	37	0	46	171	34	86	262
10	38	0	46	171	34	86	260
10	39	0	46	171	34	86	258
10	40	0	46	171	33½	86	254
10	41	0	46	171	33	86	246

{ Apparatus (G) in the bulb, in open  
air, since 9 h. 53'.

Experiment

Experiment VI. made January 11, 1782.

Time per Watch.		Standard spirit H.	Mercurial therm. C.	Spirit therm. D.	Mercurial therm. B.	Remarks and Occurrences.	A in air, Mer. therm.
h.	"						
10	42	0	46	171	32½	86	244
10	43	0	46	170	32½	86	238
10	44	0	46	170	32½	86	234
10	45	0	46	170	32½	86	230
10	46	0	46	170	32½	86	228
10	47	0	46	170	32½	86 { External part of quicksilver in the apparatus (F) fluid, the center a globular solid. In the open air an hour. }	224
10	49	0	46	170	32½	86	214
10	50	0	46	170	32½	86	213
10	51	0	46	170	32½	86	208
10	52	0	46	170	32	86	206
10	53	0	46	170	32	86	200
10	54	0	45	170	32	86	196
10	55	0	45	170	32	86	192
10	56	0	45	170	32	86	190
10	57	0	45	170	32	86	186
10	58	0	45	170	31½	86	176
10	59	0	45	170	31½	86	166
11	0	0	45	170	31½	86 { A portion of quicksilver in apparatus (F) still frozen. }	160
11	5	0	44	170	31	85	134
11	6	0	44	170	31	85	128
11	7	0	44	170	31	85	126
11	8	0	44	170	31½	86	124
11	9	0	44	170	31½	86	116
11	10	0	44	170	31½	86	116
11	11	0	44	170	31½	86	110
11	12	0	44	170	31½	86	100
11	13	0	44	170	31½	86	96
11	14	0	44	170	31½	86	92
11	15	0	44	170	31½	86	84
11	16	0	44	170	31½	86	82
11	17	0	44	170	31	85	78
11	18	0	44	170	30½	85	68
11	19	0	44	166	30½	85	60
11	20	0	44	167	30	85	54

Experiment VI. made January 11, 1782.

Time per Watch..		Standard Spirit H.	Mercurial therm C.	Spirit therm. D.	Mercurial therm. B.	Remarks and observations.	Mer. ther. A in air.
h.	' "						
11	22	0	43	167	30	85	48
11	23	0	43	167	30	85	44
11	25	0	43	167	30	85	42
11	26	0	43	167	30	85	40
11	27	0	43	167	30	85	40
11	28	0	43	167	30	85	40
11	29	0	43	167	30	85	40
11	30	0	43	167	30	85	40
11	31	0	43	167	30	85	40
11	32	0	43	167	30	85	38
11	33	0	43	167	29½	85	36
11	34	0	43	167	29½	85	36
11	35	0	43	167	29½	85	36
11	36	0	43	167	29½	85	36
11	37	0	43	167	29½	85	36
11	38	0	43	167	29½	85	36
11	39	0	43	167	29½	85	36
11	40	0	43	167	29½	85	36
11	42	0	43	167	29½	85	36
11	44	0	43	167	29½	85	36
11	46	0	43	167	29½	85	34
11	48	0	42½	166	29	85	34
11	49	0	42	165	28½	434 { The quicksilver in thermometer (B) sunk in an instant. }	34
11	51	0	42	165	28½	432	34
11	52	0	42	165	28½	432	34
11	53	0	42	165	28½	432	34
11	53	30	42	165	28	430	34
11	54	0	42	166	28	427	
11	55	0	42	315	28	425 { The quicksilver in thermometer (C) subsided all at once. }	
11	56	0	41	360	28	422	
11	57	0	41	360	28	420	
11	58	0	40½	358	28	417	
11	58	30	40½	355	27½	415	
11	59	0	40	352	27½	413	

Experiment

Experiment VI. made January 11, 1782.

Time <i>per</i> Watch.			Standard spirit H.	Mercurial therm. C.	Spirit therm. D.	Mercurial therm. B.	Remarks and Occurrences.
h.	'	"					
12	0	0	40	350	27 $\frac{1}{4}$	408	
12	1	0	40	345	27 $\frac{1}{4}$	404	
12	2	0	40	336	27	398	
12	3	0	40	330	27	394	
12	4	0	40	323	27	390	
12	5	0	40	315	27	384	
12	6	0	40	309	27	379	
12	7	0	40	300	27	374	
12	8	0	40	294	27	368	
12	9	0	40	285	27	362	
12	10	0	40	275	27	355	
12	12	0	40	257	27	343	
12	13	0	40	245	27	335	
12	14	0	40	235	26 $\frac{1}{2}$	329	
12	15	0	40	222	26 $\frac{1}{2}$	320	Made a fresh mixture.
12	16	0	—	—	—	—	Put the instruments into the fresh mixture.
12	16	30	40 $\frac{1}{2}$	270	33	340	
12	17	0	42	335	33	340	
12	17	30	42	365	33	340	
12	18	0	42	370	33 $\frac{1}{2}$	340	
12	18	30	42	372	33 $\frac{1}{2}$	340	
12	19	0	42 $\frac{1}{2}$	372	33 $\frac{1}{2}$	340	
12	20	0	43	372	33 $\frac{1}{2}$	340	
12	21	0	43	372	33 $\frac{1}{2}$	340	
12	22	0	43	372	33 $\frac{1}{2}$	340	
12	24	0	43	374	33 $\frac{1}{2}$	340	
12	26	0	43	374	33	340	
12	28	0	43	374	33	340	
12	32	0	43	374	33	340	
12	37	0	43	374	33	340	
12	42	0	43	374	32	340	
12	45	0	43	374	32	340	
12	54	0	43	374	32	340	
12	59	0	43	374	32	340	
1	0	0	43	374	32	340	

Experiment VI. made January 11, 1782.

Time per Watch.			Standard spirit H.	Mercurial therm. C.	Spirit therm. D.	Mercurial therm. B.	Remarks and Occurrences..
h.	'	"					
1	5	0	42	373	30	340	
1	10	0	42	371	29	340	
1	14	0	41 $\frac{1}{2}$	370	29	340	
1	19	0	41	370	28 $\frac{1}{2}$	340	
1	21	0	41	370	29	340	
1	23	0	41	370	29	340	
1	26	0	41	370	28 $\frac{1}{2}$	340	
1	30	0	40 $\frac{1}{2}$	370	28	340	
1	35	0	40	370	28	340	
1	40	0	40	370	28	340	
1	45	0	40	360	27	340	
1	46	30	40	—	27	400	
1	46	45	40	—	27	410	
1	47	0	40	—	27	438	
1	49	0	39	335	26 $\frac{1}{2}$	433	
1	50	0	39	322	26	428	
1	51	0	38	312	26	423	
1	54	0	38	291	26 $\frac{1}{2}$	413	
1	55	0	38	280	26 $\frac{1}{2}$	408	
1	56	0	38	273	26 $\frac{1}{2}$	403	
1	57	0	38	266	26 $\frac{1}{2}$	398	
1	58	0	38	259	26 $\frac{1}{2}$	394	
1	59	0	38	253	26 $\frac{1}{2}$	388	
2	0	0	38	241	26 $\frac{1}{2}$	382	
2	1	0	38	232	26 $\frac{1}{2}$	376	
2	2	0	38	225	26 $\frac{1}{2}$	371	
2	3	0	38	212	26 $\frac{1}{2}$	363	
2	4	0	38	202	26 $\frac{1}{2}$	357	
2	5	0	38	193	26 $\frac{1}{2}$	350	
2	6	0	37 $\frac{1}{2}$	180	26 $\frac{1}{2}$	341	
2	7	0	37 $\frac{1}{2}$	167	26 $\frac{1}{2}$	332	
2	8	0	37 $\frac{1}{2}$	158	26	328	
2	9	0	38	145	26	320	
2	10	0	38	132	26	310	
2	11	0	38	123	26	304	
2	12	0	38	105	26	300	

Added snow, the mixture growing thin.

Experiment

Experiment VI. made January 11, 1782.

Time per Watch.		Standard spirit H.	therm. C. Mercurial	Spirit therm. D.	Mercurial therm. B.	Remarks and Occurrences.
h.	' "					
2	13	0	38	80	26	282
2	14	0	38	55	26	276
2	15	0	38	37	26	270
2	16	0	38	45	26	262
2	17	0	38	35	26	252
2	18	0	38	35	26	250
2	19	0	37½	34	26	244
						Took out all the instruments.

This singular experiment, though it did not answer the intention for which it was principally designed, yet afforded many striking phænomena which I shall mention in the course of these remarks. After a cold night, the quicksilver in the thermometer was at 44° below 0 at seven o'clock in the morning: thinking this great degree of cold was the most favourable opportunity of observing how low it was possible to make the quicksilver descend in the tube of the thermometers, I resolved to embrace it, and at the same time to observe the concurrent degrees with a spirit thermometer; but as those sent out to me in 1781 (D and E) differed so much from the thermometers of quicksilver, I resolved to make use of another spirit thermometer made by NAIRNE and BLOUNT, and which was also furnished me by the Royal Society in 1774. With this instrument, which I call the standard, and marked with the letter H, I have made observations eight years, and found it agree very well with others made of quicksilver; and the more readily to discover the variation of (D), I employed it also in the same experiment; but before I began the following observations

\*A a a z

were



were taken, the instruments all exposed to the open air, where they are continually kept. The thermometers are marked from (A) to (H), and the observations are regularly in that order.

h. ,	A	B	C	D	E	F	G	H
7 45	44½	45	41	28	29	40	40	46
7 50	46	64	124	30	32	42	41	46
7 55	—	—	60	—	—	—	—	—
7 57	44	—	—	—	—	—	—	—

It is observable, that neither the quicksilver which was in the cylinders affixed to (F) and (G), nor the other quicksilver which I constantly kept in the same place, some in a faucer, some in a gallipot, and some in a phial, shewed the least appearance of congelation. Being engaged in preparing for the ensuing experiment, I did not remark either the great descent or ascent of the quicksilver in (C), which must have been very sudden, as my remarks are only five minutes asunder.

It may be necessary to mention, that the thermometer (H) was mounted on a scale the whole length (as usual for meteorological observations), and (C) was armed with elastic gum from the bulb to about half or three-quarters of an inch above the surface of the freezing mixture.

The small descent of the quicksilver in (C), and the little effect produced by moving it into a second mixture, made me at first apprehend the instrument was damaged; I did not, however, take it out, but took another thermometer (A), and put it also in the mixture; but I find it was stationary at a higher degree than (C): I therefore exchanged (A) for the mercurial thermometer (B), which to my great surprize was stationary at 86°, nor could it be got lower until the cold of the mixture diminishing it fell at

at once to  $434^{\circ}$ , and a few minutes afterwards (C) fell to  $360^{\circ}$ . Imagining that a new mixture would now bring it very low, I made another, but in the mean time the instruments had risen greatly, and after standing in the fresh mixture (C) sunk to  $374^{\circ}$ , and (B) to  $438^{\circ}$ . I should have mentioned, that these mixtures were double in quantity to those used in the former experiments; instead of glass tumblers, they were made in pint basons.

I observed also, that the mixtures seemed to grow thin sooner than common; for I always made them of the consistence of pap. I added snow at times, to thicken it, but found it had very little effect, but rather decreased the cold. It is with great diffidence I offer it as my opinion, that the temperature of the air was too cold, and that the quicksilver being nearly in a state of congelation before plunged into the mixture, was instantly frozen on putting the instruments into them; and as the quicksilver in the tubes must have been of the same temperature with that in the bulbs of the thermometers before the experiment, I should imagine, that when the quicksilver in the bulb was frozen solid, it communicated an addition of cold to that in the tube, and froze it also, which prevented its subsiding as usual; for in other cases, the contraction of the quicksilver, when solid in the bulb, was the cause of the quicksilver subsiding in the tube; but then the latter was fluid, for the circumambient air was warmer than the degree at which quicksilver freezes, and the increased cold was applied only to the bulb. The observations made before the experiment began, as related in the beginning of these remarks, shew the quicksilver in the thermometer was congealing, and that (A) and (C) were actually frozen.

When

When I removed the thermometer (A) out of the mixture at 9 h. 5', I hung it up in the air, and have noted down, in a separate column on the right-hand side of the page, its appearances corresponding to the times put down on the other side of the page. It is remarkable, that (A) and (C) have each an air-bubble blown at the top; but the thermometer (B) had none.

Whilst the instruments were stationary in the foregoing experiment, I put the apparatus (F) and (G) severally into the mixture with the others; the consequence was, that in two minutes the quicksilver in the cylinder was frozen solid; but as there was a difference in the effect I shall be more particular. At 9 h. 48' put in apparatus (F), when it stood in the air at  $40^{\circ}$  or  $41^{\circ}$  below 0; and at 9 h. 50' took it out frozen solid, and the inclosed thermometer pointing still at  $40^{\circ}$  or  $41^{\circ}$ . I then hung it up in the open air, and looked at it only now and then. At 10 h. 47' (after being exposed to the air near an hour), I found only a small quantity of the surface of the quicksilver was fluid, the rest was a frozen globe resembling a ball of polished silver; the thermometer inclosed was still at  $40^{\circ}$ . At 11 h. 4' I observed a segment of a globe of solid quicksilver; in the inside was a concavity made, I supposed, by the bulb of the thermometer. The thermometer was still at  $40^{\circ}$ , which undoubtedly is the freezing point of quicksilver, as in this instance part of it was frozen, and part solid. I withdrew the thermometer, poured out the fluid quicksilver, and returned the thermometer into the cylinder, shortly after which it was at  $37^{\circ}$ , and the frozen segment was then fluid.

The apparatus (G) was hanging in the open air at  $40^{\circ}$ , and put into the same freezing mixture at 9 h. 51', on which it sunk instantly to  $210^{\circ}$ , at which degree it was stationary at  
9 h.

9 h. 53', when it was taken out of the mixture perfectly solid. At 10 h. 6' I saw it had subsided into the bulb (I mean the quicksilver in the inclosed thermometer) which was the last time I particularly noticed it. It may be necessary to mention, that finding the quicksilver in the enclosed thermometer sink instantaneously as soon as the apparatus was put into the freezing mixture, I took it out immediately, to view it, and replaced it in a few seconds of time. I found the quicksilver was not yet solid, but was in frozen pieces of irregular shapes, resembling ice that had been broken to pieces by concussion in a pail of water, but with this remarkable difference, that as ice swims on the water, the frozen quicksilver subsided in fluid quicksilver, and the segment of ice, mentioned a little before to be found in the thermometer (F) was also at the bottom of the cylinder, and remained there after decanting the liquid quicksilver from it. Hence we may conclude, that cold increases the gravity of quicksilver, as indeed must be the case, since it is certain it occupies less space in a solid than in a fluid state.

Experiment

Experiment VII. made January 22, 1782.

Time per Watch.	Standard spirit H.	Mercurial therm. C.	Spirit therm. F.	Remarks and Occurrences.
h. ' "				
8 42 ' "	—	—	—	Making the fresh mixture.
8 45 ' 0	—	—	—	Put in the instruments.
8 45 30	38	65	31½	
8 46 ' 0	41	105	33	
8 46 15	42	130	33	
8 46 30	43	155	33½	
8 46 45	44	183	33½	
8 47 15	44	207	33½	
8 47 45	44	235	34	
8 48 ' 0	44½	235	34	
8 48 30	45	235	34	
8 49 ' 0	45	235	34	
8 50 ' 0	45	235	34	
8 51 ' 0	45	235	34	
8 52 ' 0	46	237	34	
8 53 ' 0	46	238	34	
8 54 ' 0	46	238	34	
8 55 ' 0	46	238	34	
8 56 ' 0	46	237	34	
8 57 ' 0	46	237	34	
8 58 ' 0	46	236	34	
9 ' 0 ' 0	46	236	34	Making a fresh mixture.
9 4 ' 0	45½	236	33	
9 5 ' 0	—	—	—	Removed the instruments into the new mixture.
9 5 15	44	235	32	
9 5 45	44	237	32	
9 6 ' 0	44½	237	31	
9 8 ' 0	—	—	—	Put in apparatus (G).
9 9 ' 0	—	—	—	{ (G) sunk into the bulb, quicksilver in the cylinder fluid.
9 11 ' 0	44½	237	30½	
9 13 ' 0	44½	237	29½	
9 15 ' 0	44½	237	29½	
9 16 ' 0	44½	237	29½	
9 17 ' 0	44	237	29½	
9 18 ' 0	44	237	29½	
9 19 ' 0	43½	237	29½	

Experiment VII. made January 22, 1782,

Time per Watch.		Standard spirit H.	Mercurial therm. C.	Spirit therm. D.	Remarks and Occurrences.
h.	"				
9 20	0	43½	237	29½	Examined (G) remains as the last time.
9 21	0	43½	237	29	
9 22	0	33	236	29	
9 23	0	42½	236	29	
9 24	0	42½	235	29	
9 25	0	42½	235	29	
9 26	0	42½	235	29	
9 27	0	42	235	29	
9 28	0	42	234½	29	
9 29	0	42	234	29	
9 30	0	42	234	29	(G) remains still the same.
9 31	0	41½	234	29	
9 32	0	41	234	29	
9 33	0	41	234	29	
9 34	0	41	234	28½	
9 35	0	41	234	28½	
9 36	0	41	234	28½	
9 37	0	40½	234	28½	
9 38	0	40½	234	28	
9 39	0	40½	234	27½	
9 40	0	40	234	27	(G) remains in the bulb, quick. in the cylinder fluid.
9 41	0	40	234	27	
9 42	0	40	234	26½	
9 43	0	40	233	26½	
9 44	0	40	232	26½	
9 45	0	40	232	26½	
9 46	0	39½	232	26½	
9 47	0	39½	232	26½	
9 48	0	39	232	26	
9 49	0	39	231½	26	
9 50	0	39	231½	26	
9 50	30	39½	231	26	
9 51	0	39	232	26	
9 52	0	39	650	26	
9 52	20	—	850	—	
9 52	30	—	1050	—	
9 52	40	—	1120	—	

Experiment VII. made January 22, 1782.

Time per Watch.	Standard Spirit H.	Mercurial therm. C.	Spirit therm. D.	Remarks and Occurrences.
h. ' "				
9 52 50	—	1300	—	
9 53 0	—	1350	—	
9 53 10	—	1300	—	
9 53 30	38	1361	26	
9 54 0	38	1361	26	Apparatus (G) as before; took it out entirely.
9 56 0	38	1362	25½	
9 57 0	38	1362	25½	Made a fresh mixture.
9 58 30	38	1305	31	Put the instruments into the new mixture.
9 59 30	39	1305	32	
10 0 0	40	1305	32	
10 0 30	40	1305	32	
10 1 0	40	1306	32	
10 2 0	40	1306	32	
10 3 0	40	1306	32	
10 4 0	40	1307	32	
10 5 0	40	1306	32	
10 6 0	40	1306	31½	
10 7 0	40	1306	31½	
10 8 0	40	1306	31½	
10 9 0	40	1307	31	
10 10 0	40	1307	31	
10 11 0	40	1307	31	
10 12 0	39½	1307	30½	
10 13 0	39½	1307	30½	
10 14 0	39	1306	30	
0 15 0	39	1306	30	Found (C) has lost its bulb in the former mixture.

*Remarks on the seventh experiment.*

From the sixth experiment I was induced to think, that the nearer the temperature of the atmosphere approached to the freezing point of quicksilver, so that a great degree of cold might be communicated to the bulb of a thermometer and yet the quicksilver in the tube remain fluid, would be the properest

perest time for ascertaining in this manner to what degree quicksilver will contract by the application of cold. With this view this seventh experiment was made: the several thermometers from A to H were as follows, before I began, A 38, B 36, C 33, D 24, E 24½, F 33, G 33, H 37. Those used in the experiment were C, D and H. The first was to shew the descent of the quicksilver; and the two last, which were spirit thermometers, were employed to shew the corresponding contractions of the two substances, quicksilver and alcohol. After above an hour's attendance on them, I was highly pleased to see the quicksilver fall to 1367° below the cypher, especially as I supposed, by changing the mixture for a fresh one, I should get it much lower still. I made another accordingly, and removed the instruments into it. The quicksilver rose, as was common in changing the mixtures; but after waiting a considerable time, without its descending again, I recollected Professor BRAUN mentioning that his thermometers were always broken when below 600°. This made me examine mine, and I found the bulb was broken and fallen off; and on a diligent search in the mixture, I could not find either quicksilver or the pieces of glass; I therefore conclude it had dropped off into the other mixture, which unluckily I had thrown away the moment before, having occasion to use the basin in decanting the present mixture: I have no doubt but it broke at the time the quicksilver fell so rapidly. During the course of this experiment I put the apparatus (G) into the freezing mixture; in a minute's time the quicksilver in the inclosed thermometer had subsided into the bulb, and remained so during the time it continued immersed in the freezing mixture, which was about three quarters of an hour; but though the thermometer, which made part of the apparatus, shewed so great a degree of cold,

\*B b b 2

yet



yet the quicksilver in the cylinder was never frozen ; and indeed the spirit thermometers, suspended in the mixture, seemed to indicate, that there was not sufficient cold to freeze quicksilver, except at the beginning ; for I observe, it is not effected at  $40^{\circ}$ , without continuing some time at that degree, as appears very clearly from the third experiment.

Experiment VIII. made December 21, 1781.

Time per Watch.	Mercurial thermom.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. m. s.				
10 10 0	—	—	—	Made the freezing mixture.
10 12 0	—	—	—	Put in the gallipot of quicksilver.
10 13 0	—	—	—	Put thermometer (B) into the quicksilver.
10 13 30	—	35	—	
10 13 35	—	36	—	
10 13 40	—	37	—	
10 14 0	—	37	—	Put thermometer (A) into the freezing mixture.
10 14 30	30	38	—	
10 14 40	40	38	—	
10 15 0	42	38	—	
10 15 15	43	39	—	
10 15 30	44	39	—	
10 16 0	48	39	—	
10 16 15	56	39	—	
10 16 30	64	39	—	
10 16 45	70	39	—	
10 17 0	76	39	—	
10 17 15	81	39	—	
10 17 30	87	39	—	
10 17 45	92	39	—	
10 18 0	97	39	—	
10 18 15	102	39	—	
10 18 30	108	39	—	
10 18 45	113	39	—	
10 19 0	118	39	—	
10 19 15	122	39	—	
10 19 30	126	39	—	
10 19 45	130	39	—	

Experiment

Experiment VIII. made December 21, 1781.

Time per Watch.			Mercurial thermom.	Apparatus.	Spirit thermom.	Remarks and Occurrences.
h.	'	"				
10	20	0	136	39	—	
10	20	15	139	39	—	
10	20	30	143	39	—	
10	20	45	147	39	—	
10	21	0	151	39	—	
10	21	15	154	39	—	
10	21	30	159	39	—	
10	21	45	163	39	—	
10	22	0	166	39	—	
10	22	15	169	39	—	
10	22	30	173	39	—	
10	22	45	175	39	—	
10	23	0	178	39	—	
10	23	15	182	39	—	
10	23	30	185	39	—	
10	23	45	187	39	—	
10	24	0	189	39	—	
10	24	15	192	39	—	
10	24	45	197	39	—	
10	25	0	199	39	—	
10	25	15	201	39	—	
10	25	45	188	39	—	{ Thermometer (A) slipped into the gallipot containing the quicksilver, by accident; replaced it.
10	27	15	200	39	—	
10	30	45	127	39	—	
10	31	0	122	39	—	
10	32	0	—	—	—	Put a spirit thermometer into the mixture.
10	32	30	96	39	27	
10	33	0	86	39	27	
10	33	30	80	39	27½	
10	33	45	72	40	27½	
10	34	30	58	40	27	Made another mixture.
10	38	45	38	40	27	Took out the instruments.
10	40	0	—	—	—	Changed the mixture.
10	40	30	59	41	—	
10	40	45	63	41	—	
10	41	0	67	41½	—	

Experiment VIII. made December 21, 1781.

Time per Watch.			Mercurial thermometer.	Apparatus.	Spirit thermometer.	Remarks and Occurrences.
h.	'	"				
10	41	10	70	41½	—	Put in the spirit thermometer.
10	41	30	78	41½	29	
10	42	0	84	41½	30½	
10	42	15	88	41½	31	
10	42	30	91	41½	31	
10	43	0	95	41	31	
10	43	15	99	41	31	
10	43	30	100	41	31	
10	43	45	101	41	31	
10	44	0	101	41	31	
10	44	30	101	41	31	
10	45	15	101	40½	31	
10	46	0	101	40	30½	
10	47	0	101	40	30½	
10	47	30	101	40	30½	
10	48	0	101	40	30½	
10	48	30	101	40	30	
10	49	0	101	40	30	
10	49	30	101	40	29½	
10	50	0	101	40	29½	
10	50	30	101	40	29½	
10	51	0	101	39½	29½	
10	51	30	101	39½	29½	
10	52	0	101	39½	29	
10	52	30	101	39½	29	
10	53	0	101	39	29	
10	54	0	113	39	29	
10	54	30	112	39	29	
10	54	45	111	39	28½	
10	55	0	110	39	28½	
10	55	30	108	39	28½	
10	55	45	106	38½	28½	
10	56	0	104	38½	28½	
10	56	30	102	38½	28½	
10	57	0	99	38½	28½	
10	57	30	96	38½	28	
10	58	0	93	38½	27½	
						The descent in the mercurial therm. was sudden.

Experiment VIII. made December 21, 1781.

Time per watch.	Mercurial thermom.	Apparatus.	Spirit thermom.	Remarks and Occurrences.
h.				
10 58 30	89	38½	27½	
10 59 0	85	38	27½	
10 59 30	82	38	27½	
11 0 0	78	38	27½	Stirred the mixture.
11 0 30	75	38	27½	
11 1 0	70	38	27½	
11 1 30	66	38	27½	
11 2 0	64	38	27½	
11 2 30	60	38	27½	Stirred the mixture.
11 3 0	55	38	27½	
11 3 30	51	37½	27	
11 4 0	45	37½	27	
11 4 30	44	37	27	

This eighth experiment was made with a view to try, whether quicksilver would freeze whilst in contact with the freezing mixture. For this purpose I did not use the apparatus employed in the other examples, but substituted another, by taking a gallipot made of flint stone (as being thinner than the common sort) of about an ounce measure, and filled it half full of quicksilver, into which I inserted the mercurial thermometer (B) and employed the other mercurial thermometer (A) as an index, as before. I hoped by this means to determine exactly when the quicksilver was congealed, as I had free access to it at all times, which was not the case when inclosed in the cylindrical glass, the worsted wound round the tube of the ivory thermometer to exclude the air, equally excluding any instrument from being introduced to touch the quicksilver. I made a kind of skewer, with a flat blunt point, of dried cedar wood

wood for lightness, which I found would remain in the gelatinous freezing mixture at any depth I chose; but when inserted into the quicksilver contained in the gallipot, the great disproportion of gravity made it rebound upwards, and by the touch I could easily perceive, by the resistance it met with, whether it proceeded from quicksilver in a fluid or congealed state. The event did not answer my wishes, for I could not find that the quicksilver was frozen in the least during the trial. Indeed the temperature of the air was not favourable, being under  $20^{\circ}$  below the cypher. The large quantity too of the quicksilver in the gallipot, as well as the thickness of that vessel, might both of them contribute to render the operation unsuccessful; yet, as the apparatus thermometer shewed the same degree ( $-40$ ) as when quicksilver froze in the glass cylinder, I am of opinion it would congeal by this simple method in very cold weather, and a long continued application of a proper degree of cold by the mixtures.

Experiment IX. made February 22, 1782.

Whilst I was attending on the preceding experiment (the 5th) and had removed the instruments into a second mixture, the former one by this means being unemployed, I put into it a gallipot (the same I used in the eighth experiment) with about three quarters of a pound of quicksilver, and let it remain immersed in the mixture a considerable time (I suppose near half an hour), and finding, by touching with a quill, that part of it was congealed, I drew the gallipot out, it being previously flung with a string, and decanted off the superincumbent mixture and fluid quicksilver; the remainder, about two-thirds of the whole quantity, remained solid in the gallipot;

pot; the internal surface remained every where very rough and white, shining like an old silver spoon long in use and having lost its polish. Part of it became fluid in a few minutes; and imagining it afforded a fine opportunity of confirming what had before appeared to be the freezing point of quicksilver, I put a mercurial thermometer (F) which then stood at  $34^{\circ}$ , into the part of the quicksilver in the gallipot, which was just thawed, and it subsided directly to  $-40^{\circ}$ , and became stationary. I repeated the same with another instrument, and the consequence was the same. I then tried the spirit thermometer (D) which became stationary at  $28^{\circ}\frac{1}{2}$ ; and another spirit thermometer (E) which I took out of the freezing mixture, where it was at  $35^{\circ}$ , and it rose to  $30^{\circ}$ ; and by comparing the spirit thermometers with mercurial ones, and also with another spirit thermometer (H) it appears, that  $29^{\circ}$  on the former is about equal to  $40^{\circ}$  on the scale of both the latter. By the time these observations were taken, the frozen lump was loosened in the gallipot: I turned it out, and beat it with an hammer; it yielded a dead sound and flattened, but its cohesion was very weak; for, instead of expanding into a thin plate, as in other instances when frozen in the bulb of a thermometer, it crumbled to pieces, and had not that polish, which I had before constantly observed. I attributed these circumstances to the effect of the spirit of nitre on the quicksilver. It thawed very soon after its parts were disjoined by the stroke of the hammer.

Experiment X. made January 26, 1782.

*Quicksilver frozen by the natural cold in Hudson's Bay.*

The subject of this curious phenomenon was quicksilver put into a common two-ounce phial, and corked. The phial was about a third part full, and had been constantly standing by the thermometer for a month past. At eight o'clock this morning I observed it was frozen rather more than a quarter of an inch thick round the sides and bottom of the phial, the middle part continuing fluid. As this was a certain method to find the point of congelation, I introduced the mercurial thermometer (F) and the spirit thermometer (D) into the fluid part, after breaking off the top of the phial, and they rose directly and became stationary; the former at  $40^{\circ}$  or  $40^{\circ}\frac{1}{2}$ , the latter at  $29^{\circ}\frac{1}{4}$ , both below the cypher. Having taken these out, I put in two others, (G) and (E); the former became stationary at  $40^{\circ}$ , the latter at  $30^{\circ}$ . I then decanted the fluid quicksilver, to examine the internal surface of the frozen quicksilver, which proved very uneven, with many radii going across; some of these resembled pins with heads. Urgent business called me away an hour. On my return I found a small portion only had liquified in my absence. I then broke the phial entirely, and with a hammer repeatedly struck the quicksilver. It beat out flat, yielded a deadish sound, and became fluid in less than a minute afterwards. I should have mentioned, that I brought the thermometer (F) into a room, where it rose to  $55^{\circ}$  above the cypher, and then let it cool again in the open air, before I put it into the frozen quicksilver. My reason was, for fear the quicksilver in the thermometer should be frozen so as to render the

the observation uncertain; but I did not observe it differed any thing of consequence from (G) which was not taken in, but put directly into the phial. By the comparative observations of the several thermometers it appears, that  $30^{\circ}$  on the scale of the spirit thermometers (D) and (E) is about equal to  $40^{\circ}$  or  $41^{\circ}$  on my standard spirit thermometer (H). The following was the state of the instruments that morning,

	A.	B.	D.	E.	F.	G.	H.
At eight	-103	-80	$33\frac{1}{2}$	33	$42\frac{1}{2}$	42	46
At nine	-323	-444	-29	$-29\frac{1}{2}$	-40	-40	-44
At noon	34	32	21	$21\frac{1}{2}$	30	$29\frac{1}{2}$	34

It may be worth remarking, that the quicksilver in the thermometer (B) which had been very near  $500^{\circ}$ , and was then at  $444^{\circ}$ , very readily run up and down the tube by elevating either end of the instrument.

EXPLA-



EXPLANATION OF PLATE VII.

Fig. 1. The thermometer seen in front.

A. The stem and bulb reaching below the scale.

B. B. Worsted wrapped round the stem, in order to keep it steady in the cylinder, and prevent the access of air.

Fig. 2. The cylinder, swelled at bottom, to hold the quick-silver to be frozen.

Fig. 3. A section of the whole apparatus when put together, shewing in what manner the thermometer is inserted and retained in the cylinder. This section is perpendicular to the scale of the thermometer.

XX. *Observations on Mr. Hutchins's Experiments for determining the Degree of Cold at which Quickfilver freezes.* By Henry Cavendish, Esq. F. R. S.

Read May 1, 1783.

THE design of the following paper is to explain some particulars in the apparatus sent by me to Mr. HUTCHINS, the intention of which does not readily appear; and also to endeavour to shew the cause of some phenomena which occurred in his experiments; and point out the consequences to be drawn from them.

This apparatus was intended to determine the precise degree of cold at which quickfilver freezes: it consisted of a small mercurial thermometer, the bulb of which reached about  $2\frac{1}{2}$  inches below the scale, and was inclosed in a glass cylinder swelled at bottom into a ball, which, when used, was filled with quickfilver, so that the bulb of the thermometer was intirely furrounded with it. If this cylinder is immersed in a freezing mixture till great part of the quickfilver in it is frozen, it is evident, that the degree shewn at that time by the inclosed thermometer is the precise point at which mercury freezes; for as in this case the ball of the thermometer must be furrounded for some time with quickfilver, part of which is actually frozen, it seems impossible, that the thermometer should be sensibly above that point; and while any of the quickfilver in the cylinder remains fluid, it is impossible that it should sink sensibly

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below

below it. The ball of the thermometer was kept constantly in the middle of the swelled part of the cylinder, without danger of ever touching the sides, by means of some worsted wound round the tube. This worsted also served to prevent the access of the air to the quicksilver in the cylinder, which, if not prevented, would have made it more difficult to have communicated a sufficient degree of cold. The diameter of the bulb of the thermometer was rather less than one-fourth of an inch, that of the swelled part of the cylinder was two-thirds, so that there was no where a much less thickness of quicksilver between the ball and cylinder than one-sixth of an inch. The bulb of the thermometer was purposely made as small as it conveniently could, in order to leave a sufficient space between it and the cylinder, without making the swelled part thereof larger than necessary, which would have caused more difficulty in freezing the quicksilver in it. Two of these instruments were sent for fear of accidents.

One of the most striking circumstances in the experiments which have been made for freezing mercury, is the excessively low degree to which the thermometers sunk, and which, if it had proceeded, as was commonly supposed from the freezing mixture having actually produced such a degree of cold, would have been really astonishing. The experiments, however, made at Petersburg afforded the utmost reason to suppose, and Mr. HUTCHINS's last experiments have put beyond a possibility of doubt, that quicksilver contracts in the act of freezing, or in other words, that it takes up less room in a solid than in a fluid state; and that the very low degree to which the thermometers sunk was owing to this contraction, and not to the intensity of the cold produced: for example, in one of Mr. HUTCHINS's experiments a mercurial thermometer, placed in  
the

the freezing mixture, sunk to  $450^{\circ}$  below nothing, though the cold of the mixture was never more than  $-46$ ; so that the quicksilver was contracted not less than  $404^{\circ}$  by the action of freezing.

If a glass of water, with a thermometer in it, is exposed to the cold, the thermometer will remain perfectly stationary from the time the water begins to freeze till it is intirely congealed, and will then begin to sink again. In like manner, if a thermometer is dipped into melted tin or lead, it will remain perfectly stationary, as I know by experience, from the time the metal begins to harden round the edges of the pot till it is all become solid, when it will again begin to descend; and there was no reason to doubt that the same thing would obtain in quicksilver.

From what has been just said it was concluded, that if this apparatus was put into a freezing mixture of a sufficient coldness, the thermometer would immediately sink till the quicksilver in the cylinder began to freeze, and would then continue stationary, supposing the mixture still to keep cold enough, till it was intirely congealed. This stationary height of the thermometer is the point at which mercury freezes, though in order to make the experiment convincing, it was necessary to continue the process till so much of the quicksilver in the cylinder was frozen as to put the fact out of doubt.

If the experiment had been tried with no further precautions, I apprehended that considerable difficulties would have occurred, from want of knowing whether the cold of the mixture was sufficiently great, and when a sufficient quantity of the quicksilver was frozen; for, in the first place, there would be no judging when a sufficient quantity was frozen without taking out the apparatus now and then to examine it, which could not

be done without a loss of cold; and what is still worse, if before the experiment was completed the cold of the mixture was so much abated as to become less than that of congealing mercury, the frozen quicksilver would begin to melt, and the operator would have no way of detecting it, but by finding that great part of his labour was undone. For this reason two other mercurial thermometers were sent called A and B by Mr. HUTCHINS, the scales of which were of wood, for which reason I shall call them, for shortness, the wooden thermometers, as I shall call the two others the ivory ones, their scales being of that material; they were graduated to about  $600^{\circ}$  below nothing, and their balls were nearly equal in diameter to the swelled part of the cylinders, in order that the quicksilver in both should cool equally fast; and it was recommended to Mr. HUTCHINS to put one of these into the freezing mixture along with the apparatus: for then, if the cold of the mixture was sufficient, both thermometers would sink fast till the quicksilver in the cylinder began to freeze, when the ivory thermometer would become stationary, but the wooden one would still continue to sink, on account of the contraction of the quicksilver in its ball by freezing; but if this last thermometer, after having continued to sink for some time after the ivory one had become stationary, ceased at last to descend, it would shew, that the mixture was no longer cold enough to freeze mercury; for as long as that was the case, the wooden thermometer would continue to descend by the freezing of fresh portions of quicksilver in its ball, but would cease to do so as soon as the cold was at all less than that. As I was afraid, however, that the quicksilver might possibly freeze and stick tight in the tube of this thermometer, and prevent its sinking, which would make the cold of the mixture appear too small  
when

when in reality it was not, one of these thermometers instead of having a vacuum above the quicksilver as usual, was made with a bulb at top filled with air, in order that the pressure might serve to force down the quicksilver.

If the degree of cold at which mercury freezes had been known, a spirit thermometer would have answered better; but that was the point to be determined.

Another advantage which I expected from the wooden thermometer was, that it would afford a guess when a sufficient quantity of the quicksilver in the cylinder was frozen; for if the cold was continued long enough to make that thermometer sink to near  $400^{\circ}$  below nothing, I supposed, a very visible portion of the quicksilver would be frozen.

It must be observed, however, that in Mr. HUTCHINS's experiments the natural cold approached so near to the point of mercurial congelation, and in consequence the freezing mixture retained its cold so long as to make these precautions of not so much use as they would otherwise have been.

As it appeared, from Mr. HUTCHINS's table of comparison, that these thermometers did not agree well together, they were all examined after they came back, except the ivory thermometer F, which was broke before it arrived. This loss, however, is of little consequence, as it appeared from the above-mentioned table, that F and G agreed well together. The boiling and freezing points were first examined in the presence of Sir JOSEPH BANKS, Dr. BLAGDEN, Mr. HUTCHINS, Mr. NAIRNE, and myself, when the divisions on the scale answering thereto were found to be as follows:

Boiling

		Boiling point.		Freezing point.
A	-	220,3	-	29,9
B	-	218,8	-	30,9
G	-	215,3	-	32

The boiling point was tried in the manner recommended in the report of the Committee of the Royal Society, printed in the *Philosophical Transactions* for the year 1777, and allowance made, as there directed, for the height of the barometer at that time. In fixing the freezing point also allowance was made for the temperature of the room in which it was tried.

The great difference in the position of the boiling point on these thermometers seems owing only to care not having been taken to keep the quicksilver in the tube of the same heat as that in the ball, which is a circumstance that was very little attended to when they were made; and I am afraid is not so much observed at present as it ought to be, and which in A and B, whose tubes contained upwards of 900° of quicksilver, caused an excessively great error, and much more than it did in G, which contained fewer degrees in its tube.

In order to see whether the inequalities of the bore of the tube were properly allowed for, a column of quicksilver, about 100° long, was separated from the rest; and it was examined, whether its length comprehended the same number of degrees on the scale in different parts of the tube; when no sensible error could be found in this respect in G, and none worth regarding in B. The thermometer A, by reason of its being constructed with a bulb filled with air at top, could not be examined in this manner; but there is no reason to think, that it was faulty in this respect.

From

From what has been said it appears, that  $183^{\circ},3$  on the scale of G are equal to only  $180^{\circ}$  on a thermometer adjusted as recommended by the Committee, and therefore  $72^{\circ}$  are equal to  $70^{\circ}\frac{2}{3}$ ; so that the point of  $-40^{\circ}$  answers really to  $-38^{\circ}\frac{2}{3}$ ; that is, the cold shewn by this thermometer at the temperature of about  $-40^{\circ}$  is  $1^{\circ}\frac{2}{3}$  too great. In like manner it appears, that the cold shewn at that temperature by B is  $4^{\circ}\frac{2}{3}$ , and by A  $6^{\circ}\frac{2}{3}$ , too great.

On the whole, these thermometers seem to have been carefully made, their disagreement being owing only to a faulty manner of adjusting the boiling point, and to not allowing for the temper of the air in settling the degree of freezing; and as these points were examined after they came back, the experiments made with them are just as much to be depended on as if they had been truly adjusted at first.

These instruments were made in the year 1776, and were intended to have been sent to Mr. HUTCHINS that year, through the hands of the late Dr. MARTY, who promised to recommend the experiment to him; but, by not being got ready time enough to be sent that year, and a mistaken supposition that Mr. HUTCHINS was to come back the next summer, they were prevented from being sent till 1781; when Sir JOSEPH BANKS was informed by Mr. WEGG, that there was a gentleman at Hudson's Bay who was willing to undertake any experiments of that kind; and that the Hudson's Bay Company would be at the expence of any instruments necessary for the purpose. Then, as Sir JOSEPH thought the abovementioned apparatus well adapted to the purpose, I gladly embraced the opportunity of sending it. It appears, however, from the letter inserted by Mr. HUTCHINS, that Dr. BLACK, without being acquainted with



with what I had done, recommended nearly the same method of determining the degree of cold at which mercury freezes.

Besides the abovementioned instruments, there were sent to Mr. HUTCHINS two spirit thermometers and a thermometer marked C, made at the expence of the Hudson's Bay Company. The two spirit thermometers were made at the recommendation, and under the inspection of Dr. BLAGDEN, and were of great use, as they serve to ascertain several circumstances relating to the experiments, which could not otherwise have been determined. The intention of the thermometer C will be mentioned in the course of this paper.

Before I enter into the examination of Mr. HUTCHINS's experiments, it will be proper to take notice of a phenomenon which occurs in the freezing of water, and is now found to take place in that of quicksilver, and which occasioned many remarkable appearances in these experiments.

It is well known, that if a vessel of water, with a thermometer in it, is exposed to the cold, the thermometer will sink several degrees below the freezing point, especially if the water is covered up so as to be defended from the wind, and care is taken not to agitate it; and then, on dropping in a bit of ice, or on mere agitation, spiculæ of ice shoot suddenly through the water, and the inclosed thermometer rises quickly to the freezing point where it remains stationary\*.

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\* Though I here say conformably to the common opinion, that mere agitation may set the water a freezing, yet some experiments, lately made by Dr. BLAGDEN, seem to shew, that it has not much, if any, effect of that kind, otherwise than by bringing the water in contact with some substance colder than itself. Though in general also the ice shoots rapidly, and the inclosed thermometer rises very quick; yet I once observed it to rise very slowly, as, to the best of my remembrance, it took up not less than half a minute before it rose to the freezing point;

This shews, that water is capable of being cooled considerably below the freezing point, without any congelation taking place; and that, as soon as by any means a small part of it is made to freeze, the ice spreads rapidly through the remainder of the water. The cause of the rise of the thermometer, when the water begins to freeze, is the circumstance now pretty well known to philosophers, that all, or almost all, bodies by changing from a fluid to a solid state, or from the state of an elastic to that of an unelastic fluid, generate heat; and that cold is produced by the contrary process. This explains all the circumstances of the phenomenon perfectly well; for as soon as any part of the water freezes, heat will be generated thereby in consequence of the abovementioned law, so that the new formed ice and remaining water will be warmed, and must continue to receive heat by the freezing of fresh portions of water, till it is heated exactly to the freezing point, unless the water could become quite solid before a sufficient quantity of heat was generated to raise it to that point, which is not the case; and it is evident, that it cannot be heated above the freezing point, for as soon as it comes thereto, no more water will freeze, and consequently no more heat will be generated.

The reason why the ice spreads all over the water, instead of forming a solid lump in one part, is, that as soon as any small portion of ice is formed, the water in contact with it will be so much warmed as to be prevented from freezing; but the water at a little distance from it will still be below the freezing point, and will consequently begin to freeze.

point; but in this experiment the water was cooled not more than one or two degrees below freezing; and it should seem, that the more the water is cooled below that point, the more rapidly the ice shoots, and the inclosed thermometer rises.

If it was not for this generation of heat by the act of freezing, whenever a vessel of water, exposed to the cold, was arrived at the freezing point, and began to freeze, the whole would instantly be turned into solid ice; for as the new formed ice is not sensibly colder than water beginning to freeze, it follows, that as soon as all the water in the vessel was cooled to that point, the least addition of cold would convert the whole into ice; whereas it is well known, that though the whole vessel of water is cooled to, or even below, the freezing point, there is a long interval of time between its beginning to freeze and being intirely frozen, during all which time it does not grow at all colder.

In like manner, it is the cold generated by the melting of ice which is the cause of the long time required to thaw ice or snow. It is this also which is the cause of the cold produced by freezing mixtures; for no cold is produced by mixing snow with any substance, unless part of the snow is dissolved.

I formerly found, by adding snow to warm water, and stirring it about till all was melted, that the water was as much cooled as it would have been by the addition of the same quantity of water, rather more than  $150^{\circ}$  colder than the snow; or, in other words, somewhat more than  $150^{\circ}$  of cold are generated by the thawing of snow; and there is great reason to think, that just as much heat is produced by the freezing of water. The cold generated was exactly the same whether I used ice or snow\*.

I have

\* I am informed, that Dr. BLACK explains the abovementioned phenomena in the same manner; only, instead of using the expression, heat is generated or produced, he says, latent heat is evolved or set free; but as this expression relates to an hypothesis depending on the supposition, that the heat of bodies is owing to their  
their

I have formerly kept a thermometer in melted tin and lead till they became solid; the thermometer remained perfectly stationary from the time the metal began to harden round the sides of the pot till it was intirely solid; but I could not perceive it to sink at all below that point, and rise up to it when the metal began to harden. It is not unlikely, however, that the great difference of heat between the air and melted metal might prevent this effect from taking place; so that though I did not perceive it in those experiments, it is not unlikely that those metals, as well as water and quicksilver, may bear being cooled a little below the freezing or hardening point (for the hardening of melted metals and freezing of water seems exactly the same process) without beginning to lose their fluidity.

Mr. HUTCHINS's five first experiments were made with the apparatus, and in the manner above described. In the first experiment the ivory thermometer, inclosed in the cylinder, sunk to  $-40^{\circ}$ ; where it remained stationary for about half an hour, though the wooden thermometer, placed in the same mixture, kept sinking almost all the while. At the end of that time the apparatus was taken out of the mixture to be examined, and the quicksilver in the cylinder was found frozen. It seems evident, therefore, that the true point at which mercury freezes is  $40^{\circ}$  below nothing on the thermometer F, which was that made use of in the experiment. It cannot be lower than that,

their containing more or less of a substance called the matter of heat; and as I think Sir ISAAC NEWTON's opinion, that heat consists in the internal motion of the particles of bodies, much the most probable, I chose to use the expression, heat is generated. Mr. WILKE also, in the Transactions of the Stockholm Academy of Sciences, explains the phenomena in the same way, and makes use of an hypothesis nearly similar to that of Dr. BLACK. Dr. BLACK, as I have been informed, makes the cold produced by the thawing of snow  $140^{\circ}$ ; Mr. WILKE,  $130^{\circ}$ .

for if it was, the thermometer could not have remained so long stationary at that point, while surrounded with freezing quicksilver; and it cannot be higher, as the thermometer could not sink below the freezing point, while much of the quicksilver, with which it was surrounded, remained unfrozen.

To those who have attended to the former part of this paper it is needless saying, that the reason why the wooden thermometer continued sinking so long after the ivory thermometer became stationary is, that as the former was placed in the freezing mixture, the quicksilver in its ball froze, and therefore it continued descending during the greatest part of that half hour, by the continual freezing of fresh portions of quicksilver in its ball, and the contraction occasioned thereby; whereas the latter, which was placed only in freezing quicksilver, did not freeze.

There is a circumstance, however, in this experiment, the reason of which does not so readily appear; namely, on putting back the apparatus into the freezing mixture, after it was taken out to be examined, the thermometer sunk to  $-42^{\circ}$ ; but in about four or five minutes returned back to  $-40^{\circ}$ . The like happened on removing the apparatus into a fresh freezing mixture, and it then remained about ten minutes before it returned to  $-40^{\circ}$ . It seems probable from this, that the quicksilver in the cylinder became intirely frozen about the time that it was first taken out to be examined, and that it then grew  $2^{\circ}$  colder than the freezing point; and that this degree of cold was not sufficient to make the quicksilver in the inclosed thermometer freeze, since mercury, as was before said, will bear being cooled a little below its freezing point without freezing. What confirms this explanation is, that the spirit thermometers shew that the cold of the mixture was actually much the same as that shewn by the ivory thermometer.

In

In the second experiment, tried with the same apparatus, the ivory thermometer quickly sunk to  $-43^{\circ}$ ; but, in about half a minute, rose to  $-40^{\circ}$ , where it remained stationary for upwards of 17'. It appears, therefore, that in this experiment the quicksilver was cooled  $3^{\circ}$  below the freezing point, without losing its fluidity; it then began to freeze, and the inclosed thermometer immediately rose to  $-40^{\circ}$ : so that this experiment, besides confirming the former, shews, that quicksilver is capable of being cooled a little below the freezing point without freezing; and that it suddenly rises up to it as soon as it begins to lose its fluidity.

In this experiment the cold was carried far enough to freeze the quicksilver in the ivory thermometer, which was not the case in the former: for after it had remained 17' stationary at  $-40^{\circ}$ ; it began to sink again, and in about a minute sunk to  $-44^{\circ}\frac{1}{2}$ ; it then sunk instantaneously to  $-92^{\circ}$ , and soon after remained fixed for an hour and a quarter at  $95^{\circ}$ ; being then left without examination for three-quarters of an hour, the mercury was found to have sunk into the ball, the spirit thermometer shewing at that time that the mixture was rather above the point of freezing, whereas before it had been below it. It appears, therefore, that the quicksilver in the thermometer, after having descended to  $-44^{\circ}\frac{1}{2}$ , froze in the tube, and stuck there; but, being by some means loosened, sunk instantly to  $-92^{\circ}$ , and again stuck tight at  $-95^{\circ}$ , till at last the mixture rising above the freezing point, the quicksilver in the tube melted, and sunk into the ball, to supply the vacuum formed there by the frozen quicksilver. A similar accident of the quicksilver freezing in the tube of the thermometer, and sticking there, and then melting and sinking into the ball as the weather grew warmer, has been found by Dr. BLADEN to have.

have happened to several gentlemen whose thermometers froze by the natural cold of the atmosphere, and with reason caused much perplexity to some of them.

In this experiment the apparatus was not taken out to be examined till the ivory thermometer had sunk to  $-95^{\circ}$ ; it was then found to be frozen solid.

The third experiment was tried while the former was carrying on, and was made by putting the other apparatus, namely, that with the thermometers G and B, into the first mixture made for the former experiment, and which may consequently be supposed to have lost great part of its cold. The ivory thermometer quickly sunk to  $-43^{\circ}$ , where it remained stationary for near 12'. The apparatus being then taken out to be examined, the quicksilver in the cylinder was found fluid, but thick and in grains, like crumbs of bread. The apparatus was then put back into the mixture; and, on observing the thermometer, it was found to have risen to  $-40^{\circ}$ , where it remained stationary about 40'; being then examined, the quicksilver was found solid.

It appears, therefore, that the cold of the mixture was sufficient to cool the quicksilver in the cylinder about  $3^{\circ}$  below the point of freezing, but did not make it freeze till, on taking out the apparatus, the agitation suddenly set it a freezing, and produced the appearance described by Mr. HUTCHINS. This immediately made the inclosed thermometer rise; so that when it was re-placed in the mixture and observed, it stood exactly at the freezing point. It appeared, by the spirit thermometer, that the cold of the mixture, at the time the apparatus was first taken out to be examined, was only  $2^{\circ}$  below the point of freezing, which agrees very well with this explanation.

This

This experiment, therefore, affords a fresh confirmation that the point of mercurial congelation is  $-40^{\circ}$  on these thermometers; and that quicksilver will bear being cooled a little below that point without freezing.

As in these two experiments the quicksilver in the cylinder and ivory thermometer bore being cooled a few degrees below the freezing point without freezing, it is natural to conclude, that the same fluid in the wooden thermometer should do so too; and it may, perhaps, be supposed that, in consequence of it, this thermometer, after having sunk a little below the point of freezing, ought suddenly to have risen up to it, which was not observed. But there is great reason to think, that though the quicksilver in it did bear cooling in this manner, it would not have occasioned any such appearance: for suppose that it is cooled below the freezing point, and then suddenly freezes, its bulk will be increased, on account of the heat generated thereby; but then it will be diminished on account of the contraction in freezing; so that, unless the expansion by the heat generated exceeds the contraction by freezing it will cause no rise in the thermometer. I do not, indeed, know how much the heat generated by freezing in quicksilver is, but in water it is about  $150^{\circ}$ , and the contraction by freezing is at least as much as its expansion by  $400^{\circ}$ ; so that, unless the heat generated by freezing is two or three times as great in quicksilver as in water, the thermometer ought not to rise on this account.

In the fourth, fifth, sixth, and seventh experiments a new phenomenon occurred, namely, the ivory thermometer sunk a great deal below the freezing point without ever becoming stationary at  $-40^{\circ}$ . In the fifth experiment, tried with the apparatus G, it quickly sunk to  $-42^{\circ}$ , and then, without remaining stationary at any point, sunk in half a minute to



- 72°, and soon after remained fixed at - 79°. While it was at - 79°, the apparatus was twice examined, and the quicksilver found fluid; but being again examined after having been removed into a fresh mixture, it was found solid.

It seems likely from hence, that the quicksilver, in the cylinder was quickly cooled so much below the freezing point as to make that in the inclosed thermometer freeze, though it did not freeze itself. If so, it accounts for the appearances perfectly well; nor does there seem any thing improbable in the explanation, except that it is contrary to what happened in the three first experiments; but the degree to which fluids will bear being cooled below the freezing point without freezing seems to depend on such minute circumstances, that, I think, this forms no objection. It must be observed, that the cold of the mixture appeared by the spirit thermometer to be five or six degrees below the freezing point; so that if the quicksilver in the cylinder was as cold as the mixture, and I have no reason to think it was not, it is not at all extraordinary that the thermometer should have froze; the only thing extraordinary is, that the quicksilver in the cylinder should have borne that cold without freezing.

The same phenomenon occurred in the sixth and seventh experiments, on putting the same apparatus into the freezing mixture.

In the fourth experiment the ivory thermometer sunk quickly to - 42°; but soon after rose half a degree, probably from the cold of the mixture diminishing; it then, after having remained six or seven minutes at those two points, sunk very quick to - 77°. It does not appear, at what time the quicksilver in the cylinder began to freeze, as it was not examined till long after the thermometer had sunk to - 77°, when it was found

found solid; but from the resemblance of this to the three former experiments, I think it much most likely, that it did not begin to freeze till after the thermometer had sunk to  $-77^{\circ}$ .

In the fifth experiment the wooden thermometer was partly frozen before it was put into the freezing mixture, and the ivory one was at  $-40^{\circ}$ . On putting them into the mixture, they both rose; the latter, half a degree; the former, many degrees; which shews that the part of the mixture in which they were placed was rather warmer than the freezing point, though that in which the spirit thermometer was placed was colder; but as there seems nothing to be learnt from this, it is not worth while entering into a detail of the circumstances.

Though these experiments do not serve to shew what the freezing point of quicksilver is, yet they do not at all contradict the conclusion drawn from the three former.

If these experiments only had been made, I should have been inclined to suppose, that quicksilver froze with a less degree of cold in vacuo than in the open air, as the quicksilver in the ivory thermometer was in vacuo, and that in the cylinder was not; but, as in the three former experiments, the event was different, the quicksilver in the cylinder there freezing first, I have no reason to think that this is the case.

Though in the sixth experiment the thermometer in the apparatus G froze without the quicksilver with which it was surrounded freezing, yet in trying the apparatus F in the same mixture, this did not happen; but, on the contrary, it afforded as striking a proof that the point of freezing quicksilver answers to about  $-40^{\circ}$  on this thermometer as any of Mr. HUTCHINS's experiments; for, on taking out the apparatus after it had been two minutes in the mixture, the quicksilver in the cylinder was found frozen solid, the inclosed ther-

mometer standing at  $40^{\circ}$  or  $41^{\circ}$  below nothing. After having been exposed for near an hour to the air, which was then very little above the point of freezing quicksilver, only a small quantity of the surface was become fluid; the rest formed a frozen globe round the ball of the thermometer, resembling polished silver, and in 17' after this only a segment of a globe of frozen quicksilver, with a concavity on the inside, formed by the ball of the thermometer, was observed, the thermometer all this while continuing the same as before, namely, at  $40^{\circ}$  or  $41^{\circ}$  below nothing; so that in this experiment the ball of the thermometer was surrounded for more than an hour with quicksilver, which was visibly frozen and slowly melting, and during all which time it continued stationary at  $40^{\circ}$  or  $41^{\circ}$  below nothing.

It must be observed, however, that in the first and second experiments, which were both tried with this apparatus, the freezing point came out exactly  $-40^{\circ}$ , whereas in this it seemed about half a degree lower; the reason of which, in all probability, is, that the tube of this thermometer was not so well fitted to its scale but that it had a little play, which would make the freezing point appear near half a degree higher or lower, according as the tube was pushed up or down.

Though the foregoing experiments leave no reasonable room to doubt, that this is the true point at which quicksilver freezes, yet Mr. HUTCHINS has, if possible, made this still more evident by his two last experiments; as, in the first of them, he froze some quicksilver in a gally-pot immersed in a freezing mixture, so that the quicksilver was in contact with, and covered by, the snow and spirit of nitre; and in the latter in the open air, by the natural cold of the weather, and then dipping the ball of the thermometer into the unfrozen part, observed

what:

what degree it stood at. These experiments agree with the former in shewing the freezing point to be  $-40^{\circ}$  on the two mercurial thermometers; and also shew what degree on the spirit thermometers answers thereto, namely,  $29^{\circ}\frac{1}{4}$  or  $28^{\circ}\frac{1}{2}$  on D, and  $30^{\circ}$  on E; for in these two experiments the spirit thermometers also were dipped into the frozen quicksilver.

In all the experiments, therefore, tried with the thermometer G, the freezing point came out  $-40^{\circ}$ . In those tried with F, it came out either  $-40^{\circ}$ , or about  $-40^{\circ}\frac{1}{4}$ ; so that as it appears, from Mr. HUTCHINS's table of comparison, that F stood at a medium a quarter of a degree lower than G, the experiments made with that thermometer also shew the freezing point to be  $-40^{\circ}$  on G; and as it appeared from the examination of this thermometer after it came home, that  $-40^{\circ}$  thereon answers to  $-38^{\circ}\frac{1}{4}$ , on a thermometer adjusted in the manner recommended by the Committee of the Royal Society, it follows, that all the experiments agree in shewing that the true point at which quicksilver freezes is  $38^{\circ}\frac{1}{4}$ , or in whole numbers  $39^{\circ}$  below nothing.

From what has been said it appears, that the point at which quicksilver freezes has been determined by Mr. HUTCHINS in different ways, all perfectly satisfactory, and all agreeing in the same result. In the three first experiments the thermometer was surrounded by quicksilver, which continued freezing till it became solid. In the sixth experiment the quicksilver with which it was surrounded continued slowly melting till the whole was dissolved; and in both cases the thermometer remained stationary all the while at what we have just said to be the freezing point. In the ninth and tenth experiments, the ball of the thermometer was dipped into quicksilver, previously frozen and beginning to melt, as usually practised in settling the

freezing point on thermometers, and agreed in the same result, the quicksilver in the last experiment being frozen by the natural cold of the atmosphere; and in the former, by being immersed in, and in contact with, a freezing mixture; so that this point appears to be determined in as satisfactory a manner as can be desired; and the more so, as it seems impossible that experiments should be made with more care and attention, or more faithfully and circumstantially related than these have been. The second and third experiments also shew, that quicksilver, as well as water, can bear being cooled a little below the freezing point without freezing, and is suddenly heated to that point as soon as it begins to congeal.

*On the contraction of quicksilver in freezing.*

All these experiments prove, that quicksilver contracts or diminishes in bulk by freezing; and that the very low degrees to which the thermometers have been made to sink, is owing to this contraction, and not to the cold having been in any degree equal to that shewn by the thermometer. In the fourth experiment the thermometer A sunk to  $-45^{\circ}$ , though it appeared by the spirit thermometers that the cold of the mixture was not more than  $5^{\circ}$  or  $6^{\circ}$  below the point of freezing quicksilver. In the first experiment also, it sunk to  $-44.8^{\circ}$ , at a time when the cold of the mixture was only  $2^{\circ}\frac{1}{2}$  below that point; so that it appears, that the contraction of quicksilver, by freezing, must be at least equal to its expansion by  $40.4^{\circ}$  of heat\*. This, how-

\* The numbers here given are those shewn by the thermometer without any correction; but if a proper allowance is made for the error of that instrument it will appear, that the true contraction was  $25^{\circ}$  less than here set down, and from the manner in which thermometers have been usually adjusted, it is likely, that in the following experiment of Mr. HUTCHINS, as well as those of Professor BRAUN, the true contraction might equally fall short of that shewn by observation.

ever, is not the whole contraction which it suffers; for it appears, by an extract which Mr. HUTCHINS was so good as to give me from a meteorological journal, kept by him at Albany Fort, that his thermometer once sunk to  $490^{\circ}$  below nothing, though it appeared, by a spirit thermometer, that the cold scarcely exceeded the point of freezing quicksilver. There are two experiments also of Professor BRAUN, in which the thermometer sunk to  $544^{\circ}$  and  $556^{\circ}$  below nothing, which is the greatest descent he ever observed without the ball being cracked. It is not indeed known how cold his mixtures were; but from Mr. HUTCHINS's, there is great reason to think that they could not be many degrees below  $-40^{\circ}$ . If so, the contraction which quicksilver suffers in freezing is sometimes not much less than its expansion by  $500^{\circ}$  or  $510^{\circ}$  of heat, that is almost  $\frac{1}{11}$  of its whole bulk, and in all probability is never much more than that.

It is very likely, however, that the contraction which quicksilver suffers in freezing is no very determinate quantity; for a considerable difference may frequently be observed in the specific gravity of the same piece of metal, cast different times over, and almost all cast metals become heavier by hammering; and it is likely that the same thing may obtain in quicksilver, which is only a metal which melts with a much less degree of heat than the rest. I do not know, indeed, how much this variation can amount to; but, on casting the same piece of tin three times over, I found its density to vary from 7,252 to 7,294, though I have great reason to think that no hollows were left in it, and that only a small part of this difference could proceed from the error of the experiment. This variation of density is as much as is produced in quicksilver by an alteration of  $66^{\circ}$  of heat; and it is not unlikely, that the descent of a thermometer, on account of the contraction of the quicksilver in its ball by freezing, may

may vary as much in different trials, though the whole mass of quicksilver is frozen and without any vacuities.

The thermometer marked C was intended for trying how much the contraction of quicksilver is; but the experiments made with it were not attended with success, as in the first experiment it did not sink so low as A had done, owing, most likely, to the great cold of the weather which froze the quicksilver in the tube; and in the second experiment the ball broke.

*On the cold of the freezing mixtures.*

The cold produced by mixing spirit of nitre with snow is owing, as was before said, to the melting of the snow. Now, in all probability, there is a certain degree of cold in which the spirit of nitre, so far from dissolving snow, will yield out part of its own water, and suffer that to freeze, as is the case with solutions of common salt; so that if the cold of the materials before mixing is equal to this, no additional cold can be produced. If the cold of the materials is less, some increase of cold will be produced; but the total cold will be less than in the former case, since the additional cold cannot be generated without some of the snow being dissolved, and thereby weakening the acid, and making it less able to dissolve more snow; but yet the less the cold of the materials is, the greater will be the additional cold produced. This is conformable to Mr. HUTCHINS's experiments; for in the fifth experiment, in which the cold of the materials was  $-40^{\circ}$ , the additional cold produced was only  $5^{\circ}$ . In the first experiment, in which the cold of the materials was only  $-25^{\circ}$ , an addition of at least  $19^{\circ}$  of cold was obtained; and by mixing some of the same spirit of nitre with snow in this climate, when the heat of the materials

materials was  $+26^{\circ}$ , I have sunk the thermometer to  $-29^{\circ}$ ; so that an addition of  $55^{\circ}$  of cold was produced.

It is remarkable, that in none of Mr. HUTCHINS's experiments the cold of the mixture was more than  $6^{\circ}$  of the spirit thermometer below the point of freezing quicksilver, which is so little that it might incline one to think, that the spirit of nitre used by him was weak. This, however, was not the case, as its specific gravity at  $58^{\circ}$  of heat was 1,4923. It was able to dissolve  $\frac{1}{1,42}$  its weight of marble, and contained very little mixture of the vitriolic or marine acid: as well as I could judge from what experience I have of spirit of nitre, it was as little phlogificated as acid of that strength usually is.

But, however extraordinary it may at first appear, there is the utmost reason to think, that a rather greater degree of cold would have been obtained if the spirit of nitre had been weaker; for I found, by adding snow gradually to some of this acid, that the addition of a small quantity produced heat instead of cold; and it was not until so much was added as to increase the heat from  $28^{\circ}$  to  $51^{\circ}$ , that the addition of more snow began to produce cold; the quantity of snow required for this purpose being pretty exactly one-quarter of the weight of the spirit of nitre, and the heat of the snow and air of the room, as well as of the acid, being  $28^{\circ}$ . The reason of this is, that a great deal of heat is produced by mixing water with spirit of nitre, and the stronger the spirit is, the greater is the heat produced. Now it appears from this experiment, that before the acid was diluted, the heat produced by its union with the water formed from the melted snow was greater than the cold produced by the melting of the snow; and it was not till it was diluted by the addition of one-quarter of its weight of  
that



that substance, that the cold generated by the latter cause began to exceed the heat generated by the former. From what has been said it is evident, that the cold of a freezing mixture, made with the undiluted acid, cannot be quite so great as that of one made with the same acid, diluted with a quarter of its weight of water, supposing the acid and snow to be both at  $28^{\circ}$  of heat, and there is no reason to think, that the event will be different if they are colder; for the undiluted acid will not begin to generate cold until so much snow is dissolved as to increase its heat from  $28^{\circ}$  to  $51^{\circ}$ , so that no greater cold will be produced than would be obtained by mixing the diluted acid heated to  $51^{\circ}$  with snow of the heat of  $28^{\circ}$ . This method of adding snow gradually to an acid is much the best way. I know of finding what strength it ought to be of, in order to produce the greatest effect possible.

By means of this acid, diluted in the above-mentioned proportion, I froze the quicksilver in the thermometer called G by Mr. HUTCHINS, on the 26th of last February. I did not, indeed, break the thermometer to examine the state of the quicksilver therein; for as it sunk to  $-110^{\circ}$  it must certainly have been in part frozen; but immediately took it out, and put the spirit thermometer in its room, in order to find the cold of the mixture. It sunk only to  $-30^{\circ}$ ; but, by making allowance for the spirit in the tube being not so cold as that in the ball, it appears, that if it had not been for this cause it would have sunk to  $-35^{\circ}$ \*, which is  $5^{\circ}$  below the point of freezing, and  
is

\* As the surface of the freezing mixture answered to  $-185^{\circ}$  on the tube, there were  $155^{\circ}$  of spirit in the tube which could hardly be cooled much below the temper of the air, and which must, therefore, be warmer than that in the ball by about  $55^{\circ}$  of this thermometer, as the heat of the spirit in the ball was before  
said

is as great a degree of cold, within 1°, as was produced in any of Mr. HUTCHINS's experiments.

In this experiment the thermometer G sunk very rapidly; and, as far as I could perceive, without stopping at any intermediate point, till it came to the above-mentioned degree of  $-110^{\circ}$ , where it stuck. The materials used in making the mixture were previously cooled, by means of salt and snow, to near nothing; the temper of the air was between  $20^{\circ}$  and  $25^{\circ}$ ; the quantity of acid used was  $4\frac{1}{2}$  oz.; and the glass in which the mixture was made was surrounded with wool, and placed in a wooden box, to prevent its losing its cold so fast as it would otherwise have done.

Some weeks before this, I made a freezing mixture with some spirit of nitre, much stronger than that used in the foregoing experiment, though not quite so strong as the undiluted acid, in which the cold was less intense by  $4^{\circ}\frac{1}{2}$ , as the thermometer G sunk to  $-40^{\circ}\frac{1}{2}$ . It is true, that the temper of the air was much less cold, namely,  $35^{\circ}$ ; but the spirit of nitre was at least as cold, and the snow not much less so. The experiment was tried in the same vessel and with the same precautions as the former.

The cold produced by mixing oil of vitriol, properly diluted with snow, is not so great as that procured by spirit of nitre, though it seems not to differ from it by so much as  $8^{\circ}$ ; for a freezing mixture, prepared with diluted oil of vitriol, whose

said to be  $-35^{\circ}$ , and the temper of the air above  $+20$ . Therefore, the correction must be equal to the expansion of a column of spirits  $155^{\circ}$  long, by an alteration of heat equal to  $55^{\circ}$  on this thermometer, which, if  $1^{\circ}$  on the scale answers to  $\frac{1}{1700}$ th of the bulk of the spirit, is equal to  $\frac{55 \times 155}{1700}$  or  $5^{\circ}$ .

specific gravity, at 60° of heat, was 1,5642, sunk the thermometer G to - 37°, the experiment being tried at the same time, and with the same precautions, as the foregoing. It was previously found, by adding snow gradually to some of this acid, as was done by the spirit of nitre, that it was a little, but not much stronger than it ought to be, in order to produce the greatest effect.

XXI. *History of the Congelation of Quicksilver.* By Charles Blagden, M. D. F. R. S. Physician to the Army.

Read June 5, 1783.

THE late experiments at Hudson's Bay have determined a point, upon which philosophers not only were much divided in their opinion, but also entertained, in general, very erroneous sentiments. Though many obvious circumstances rendered it improbable, that the term of mercurial congelation should be five or six hundred degrees below 0 of FAHRENHEIT'S scale, as had been at first supposed; yet scarcely any one ventured to imagine that it was short of 100°. Mr. HUTCHINS; however, has clearly proved, that even this number is far beyond the truth; and that quicksilver freezes in a degree of cold not exceeding that which sometimes occurs in the northern parts of Europe, and frequently in the more rigorous climates of Asia and America. It now appears, that quicksilver, so far from containing any essential principle of fluidity, does not differ from some of the other metals, in its melting point, nearly so much as they differ among themselves; and as it is malleable in its solid state, and after calcination recovers its metallic form by heat alone, without the addition of inflammable matter, there can be no doubt but it must be ranked among the perfect metals, which, therefore, arranged according

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ing to their specific gravity, are these four: platina \*, gold, quicksilver, and silver.

In the general progress of science, it is always useful at intervals, and especially when any considerable advance has been made, to look round and contemplate the prospect left behind. Thus our actual situation is more distinctly comprehended, and a better judgement may be formed of what remains to be done. For this reason, I thought it might not be unacceptable to the Society, if I were, on the present occasion, to lay before them an account of the different observations and experiments I have been able to collect, relative to the congelation of quicksilver; especially as many of these are recorded in books not easily procured, and in languages little understood by the learned of this country. I shall begin with the various attempts which have been made to render this metal solid by frigorific mixtures, and then enumerate the many instances in which that effect has certainly been produced by the natural cold of the air.

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## P A R T I.

§ 1. IT was undoubtedly M. JOSEPH ADAM BRAUN, Professor of Philosophy in the Imperial Academy at Petersburg, who first, upon decisive evidence, established the fact, that quicksilver can be made solid by a diminution of its heat: but,

\* Unless, indeed, the irreducible black powder, obtained by M. TILLET, should be thought to place platina among the imperfect metals. See Mem. de l'Ac. Roy. des Scienc. 1779, p. 404, &c.

as in most discoveries, much depends upon accident, so, on this occasion, M. BRAUN undertook the experiments for a very different object from that which presented itself in the course of them, and at the suggestion of another person. This gentleman was Dr. JOHN ERNEST ZEIHNER, Professor of Mechanics in the same Academy, who having repeated FAHRENHEIT's experiments with frigorific mixtures in Germany, before he came to settle at Petersburg, wished to try whether they might not be prosecuted further in the great natural cold which sometimes prevails in that city. Illness prevented Dr. ZEIHNER from carrying his ideas into execution; he therefore communicated them to Professor BRAUN, who was already much conversant in thermometrical experiments, and engaged him to take up the subject of artificial cold whenever the weather should be favourable for this purpose. A proper opportunity occurred on the 14th of December, 1759, O. S. the thermometer sinking in the open air so low as  $-34^{\circ}$  of FAHRENHEIT's scale, which we now know to be within a few degrees of the point at which mercury freezes. M. BRAUN accordingly prepared a frigorific mixture with *aqua fortis* and pounded ice, by means of which his thermometer was reduced to  $-69^{\circ}$ , lower, by almost 30 degrees, than it had fallen in any preceding experiments of this nature.

Part of the quicksilver had now really congealed, yet so far was M. BRAUN from entertaining a suspicion of that sort, and so entirely were his views directed to another object, that he acknowledges he had well nigh desisted from all further trials, content with having thus much exceeded such eminent philosophers as FAHRENHEIT, MUSSCHENBROECK, and REAUMUR.

Animated, however, by the hope that a still greater degree of cold might be produced, he entered upon the experiment

anew ;

anew; and all his pounded ice being expended, he was fortunately obliged to substitute snow in its place. With this fresh mixture he had the satisfaction of seeing the mercury in his thermometer sink to  $-100^{\circ}$ , and in successive experiments to  $-244^{\circ}$  and  $-352^{\circ}$ . Surprised at so unexpected an event, he drew the instrument out of the mixture, and carefully examined its bulb, to see if it had received any injury; but he found it perfectly entire, and moreover perceived a much more unexpected phenomenon, that the quicksilver was fixed, and remained immoveable above 12 minutes. On repeating the same experiment with another thermometer, graduated no lower than  $-220^{\circ}$ , all the mercury sunk into the ball, and became solid as before, not beginning to re-ascend till after a still longer interval of time.

From these appearances the professor very justly concluded, that the quicksilver in both instruments had been fixed or frozen by the cold; but as the evidence was not yet complete, he only ventured to propose the congelation of mercury as a *probable truth*, at the next meeting of the Academy held three days afterwards; and in the mean time was making preparations to acquire more palpable proofs of the fact. The thermometers ordered with this view were not ready till the 25th of December O. S. when, in company with the celebrated *ÆPINUS*, Professor of Physics, he performed the experiment with similar materials, and as soon as he found the quicksilver immoveable, broke the bulb of his thermometer. Now all his doubts were removed; he obtained a solid shining metallic mass, which extended under the strokes of a pestle, in hardness rather inferior to lead, and yielding a dull dead sound like that metal. Professor *ÆPINUS* was occupied at the same time in similar experiments, employing both thermometers and simple tubes

tubes of a large bore; with which last he remarked, that the quicksilver in them fell sensibly on freezing, and assumed a concave surface; likewise, that the congealed pieces would sink in fluid mercury; all evident proofs of its great contraction. These observations were frequently repeated during the winter, with some variety in the circumstances and phænomena, by Professor BRAUN and many other persons; I find M. LOMONOSOV, Professor of Chemistry, the Apothecary-general MOEDEL, Mess. KRASE, HIMSEL, and POISSONNIER, quoted on different occasions as witnesses, they having all either performed or assisted at the experiments. Such evidence one would have imagined sufficient to place the fact beyond all controversy, and render the congelation of mercury one of the most acknowledged truths in natural philosophy.

It may not here be improper to remark, as an additional proof how much we are indebted to accident for discoveries, that if Mr. BRAUN had chanced to begin with a spirit thermometer instead of a mercurial one, we might very possibly to this day have remained ignorant that quicksilver would freeze. For since, to judge from Mr. HUTCHINS's experiments, the former would have sunk but a few degrees in the frigorific mixture, it is not improbable, that the professor, discouraged by such a failure of success in the immediate object of his pursuit, would have relinquished all further attempts of this nature.

When the season for experiments requiring cold was past, Professor BRAUN employed himself in drawing up a general account of such as he had then made, which he communicated to the Petersburg Academy on the 6th of September, 1760, O. S. and printed soon afterwards as a separate dissertation\*. Of this so copious an extract, by Dr. WATSON, is already in-

\* De admirando frigore artificiali dissertatio.



serted in the Philosophical Transactions\*, that it would be improper for me to dwell upon any further particulars. I will only observe, that though many of the circumstances mentioned by M. BRAUN were not first remarked by himself, yet the dissertation is composed entirely in his name, all the other gentlemen very generously giving up their part to him who made the original discovery, and undertook to put the whole in a fit dress to appear before the world.

Five years afterwards, Professor BRAUN again addressed the publick on the same subject, under the title of "Supplements" to his former dissertation†. Here he declares, that since the first discovery he has suffered no winter to elapse without making similar experiments, and never failed of success in freezing the quicksilver, whenever there was a proper degree of natural cold, which he states at  $-10^{\circ}$ , in order for the experiment to be complete, though some commencement of congelation might be perceived when the temperature of the air is as high as  $+2^{\circ}$ . He confirms all his former observations, and adds many others to illustrate them; among which two are very important, as coming nearer than any yet known to ascertain the real contraction that quicksilver suffers in becoming solid. At the same time it must be confessed, he has not rectified any of his former mistakes: he retains the same groundless opinions relative to the freezing point of the quicksilver, the prodigious cold generated by his mixtures, and the explanation of various phenomena, which depend upon very different principles, from those to which he assigns them.

\* Vol. LII. p. 156.

† Supplementa de Congelatione Mercurii. Nov. Comment. Acad. Scient. Imperial. Petropol. tom. XI. p. 302. & seqq.

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The general state of M. BRAUN's experiments is, that with the above-mentioned frigorific mixtures, and once, when the natural cold was at  $-25^{\circ}$ , with rectified spirits and snow, he congealed the quicksilver, and discovered most of its properties in a solid state, especially that it is a real metal, which melts with a very small degree of heat. But not perceiving the necessary consequence of its great contraction in freezing, though aware of the fact, he perpetually confounded the diminution of its volume from this cause with that which is simply the effect of cold. Hence he considered, as the commencement of congelation, what was, in reality, its extreme term, or the utmost contraction which the whole would suffer in becoming solid. To this, indeed, he scarcely ever attained, owing to the various impediments that occurred from adhesion of the quicksilver in the thermometrical tube, hollows left in the bulb as it froze, portions of the mercury remaining uncongealed, and many other causes. All these being by their nature very irregular, his supposed freezing point came to be extremely uncertain, and several anomalous appearances were occasioned, which could not possibly be explained upon his mistaken supposition; but, notwithstanding such errors, the greatest part of our present knowledge on the subject of mercurial congelation is to be found in the writings of M. BRAUN, who may therefore justly be stiled the father of this branch of science.

In his supplementary treatise, the Professor engages to continue his researches, and to lay the result of them before the Academy, if they should lead to any thing new. But he did not live to accomplish his design. His original dissertation was re-printed, and the supplement first published, in the XIth tome of the *Novi Commentarii Academiae Scientiarum Petropolitanae* for the year 1765. This volume did not appear till 1767,

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and Professor BRAUN died the following year. These two treatises contain the substance of all his observations; but several further particulars, relative to the discovery, may be collected from the Philosophical Transactions\*, the History of the French Academy of Sciences†, and other literary publications of that period.

§ 2. Professor BRAUN, in his first dissertation, expressed a very commendable wish, that the experiment of congealing quicksilver might be repeated in other countries, and laments, in the supplement, that nothing of this kind had been done. It was not, however, till the year 1774, that his assertions received any sort of confirmation out of Russia, and then by a mode of experiment which did not seem to promise much success. M. JOHN FREDERIC BLUMENBACH, then a student of Physic at Gottingen, now Professor of Medicine in the same University, observing the intense cold that prevailed there in the month of January that year, took the opportunity of exposing some quicksilver to its action. As the original account of this experiment was given only in a single number of the Literary Journal of Gottingen‡, in the German language, I will here translate it as exactly as possible, several of the circumstances being very remarkable.

“On the 11th of January,” says M. BLUMENBACH, “at half after five in the evening, I put three drams of quicksilver in a small sugar-glass, and covered it with a mixture of equal parts of snow and Egyptian sal ammoniac. This mixture was put loose into the glass, so that the quicksilver lay

\* Vol. II. p. 670.

† Hist. de l'Académie des Sciences, 1760, p. 26.

‡ Göttingische Anzeigen von gelehrten Sachen. Stück 13. Jan. 29, 1774.

“perfectly

“ perfectly free, being only covered by it as with pieces of ice;  
“ the whole, together with the glafs, weighed fomewhat above  
“ an ounce. I hung it out at a window three ftories high,  
“ upon a fmall roof facing the weft, fo that the glafs was  
“ freely expofed to the north-weft; and I mixed with the fnow  
“ upon which it ftood two drams more of fal ammoniac. The  
“ fnow and fal ammoniac in the glafs foon froze in the open  
“ air to a mafs like ice: no fenfible change, however, appeared  
“ in the quickfilver that evening; but at one in the morning it  
“ was found frozen folid. It had divided into two large and  
“ four fmall pieces; of the former, one was hemifpherical  
“ and the other cylindrical, each feemingly rather above a dram  
“ in weight; the four fmall bits might amount to half a fcruple.  
“ They were all with their flat fide frozen hard to the glafs, and  
“ no where immediately touched by the mixture; their colour  
“ was a dull pale white, with a bluiſh caſt, like zinc, very  
“ different from the natural appearance of quickfilver. I wiſhed  
“ much to break the glafs immediately, and to try how theſe  
“ bodies would bear the hammer; but defiring rather to have  
“ witneſſes of ſuch a rare phænomenon I refrained. The ſpirit  
“ of wine, in an excellent thermometer made by BRANDER,  
“ ſtood at this time  $10^{\circ}$  under 0 of FAHRENHEIT’S ſcale, which  
“ was the cold of Upfal in 1740. Next morning, the 13th,  
“ about ſeven o’clock, I found that the larger hemifphere be-  
“ gan to melt, perhaps becauſe it was moſt expofed to the air,  
“ and not fo near as the others to the fal ammoniac mixture  
“ which lay beneath. In this ſtate it reſembled an amalgam,  
“ ſinking to that ſide on which the glafs was inclined, but  
“ without quitting the ſurface of the glafs, to which it was  
“ ſtill firmly congealed; the five other pieces had not yet un-  
“ dergone any alteration, but remained frozen hard, as I had  
“ obſerved

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“ observed them in the night. I now made haste to call some  
 “ of my friends, who came in time to see evidently all this  
 “ with me. They were the younger Dr. VOGEL, and Mess.  
 “ WEBER, WAGNER, and GRAUMANN. Toward eight o'clock  
 “ the cylindrical piece began to soften in the same manner as  
 “ the former, and the other four soon followed. About eight  
 “ they fell from the surface of the glass, and divided into many  
 “ fluid shining globules, which were soon lost in the inter-  
 “ stices of the frozen mixture, and re-united in part at the  
 “ bottom, being now exactly like common quicksilver.”

In this experiment at Gottingen it is not a little surprising, that the cold should have proved sufficient to effect such a congelation of the quicksilver. A mercurial thermometer would probably have stood lower than M. BLUMENBACH's of spirits; but how much cannot possibly be determined, without knowing the strength of the spirits, and after what method it was graduated. At all events, the difference could scarcely have amounted to ten degrees, which would still give the temperature of the air at least  $20^{\circ}$  above the freezing point of quicksilver. Sal ammoniac with melting snow produces  $32$  degrees of cold; may we suppose, that it has a power of increasing the cold nearly as much when it and the snow are previously cooled below that point\*? But if so, why was not the quicksilver congealed till after a period of several hours, since other frigorific mixtures begin to act almost immediately? Besides, there was not here the appearance of action, which consists in a solution of the snow, instead of its freezing into a mass. Or had the cold been greater where the mercury was placed than at M. BLUMENBACH's thermometer? The whole experiment remains

\* See Bishop WATSON's *Chemical Essays*, vol. III. p. 138.

involved in such obscurity, that some persons have supposed the quicksilver itself was not frozen, but only covered over with ice, to which opinion, however, there are great objections.

It is worthy of remark, that Gottingen, though situated in the same latitude as London, and enjoying a temperate climate in general, becomes subject at times to a great severity of cold. This of the 11th of January, 1774, is one instance. I find others when the thermometer sunk there to  $-12^{\circ}$ ,  $-16^{\circ}$ , or  $-19^{\circ}$ ; and at Cattlenburg, a small town about two German miles distant, to  $-30^{\circ}$  \*. By watching such extraordinary occasions, experiments on the freezing of quicksilver might easily be performed in many places where the possibility of them is at present little suspected. The cold observed at Glasgow in 1780 would have been fully sufficient for that purpose †.

Dr. BLUMENBACH's description of the solid quicksilver differs so much from Professor BRAUN's, with respect to its colour and general appearance, as to require a particular explanation. Their disagreement, I imagine, was occasioned by a diversity in the circumstances of their experiments. Quicksilver crystallizes in becoming solid. In this property it resembles other metallic substances, as appears from many facts, and is elegantly exemplified in those curious cups which are formed by exposing proper masses of melted metal to the cold air till the outer part be sufficiently hardened to constitute a solid coat, and then letting out the internal fluid part, so as to leave a hollow in the middle. This concavity is found every where beset with metallic crystals, scarcely yielding in beauty and regularity to

\* LAXMANN's *Sibirische Briefe*, p. 98. *Nov. Commentar. Petrop.* tom. VII. p. 396.

† Viz.  $-23^{\circ}$  on the snow,  $-14^{\circ}$  in the air. *Phil. Transf.* vol. LXX. p. 456.  
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the finest configurations of salts. In like manner, with regard to quicksilver, Professor BRAUN himself observed, that whenever it had congealed but imperfectly, and the fluid part was poured off, the solid surface which came in view was extremely rough, as if composed of many small globules. One of Mr. HUTCHINS's late observations exceedingly illustrates this matter; for he remarks, that when the fluid mercury was decanted off, in his tenth experiment, "the internal surface of the frozen quicksilver shewed very uneven, with many radii going across, some of which had heads resembling pins." Now in Professor BLUMENBACH's experiment, the quicksilver lying loose, except the flat side that touched the glass, could crystallize without impediment, and hence assumed a rough, and consequently a dead-white surface; whereas in those made by Mr. BRAUN, with tubes and thermometers, the metal being so much confined by the smooth glass, its surface was rendered of a high polish, not distinguishable in point of splendor from that of fluid mercury. Perhaps also, M. BLUMENBACH's quicksilver might have been made to look duller by some dirt or moisture collected upon it from the sal ammoniac and snow.

§ 3. In the notification of Professor BRAUN's experiments by the French Academy of Sciences, a sort of request is inserted, that proper persons might be sent by the Russians into Siberia, and by the English to Hudson's Bay, for the purpose of repeating them with every advantage of natural cold. I do not find that the Russians have taken any notice of this, and indeed with regard to them it would be almost superfluous, as scarcely a winter passes at Petersburg in which the weather is not severe enough for the ready congelation of mercury by artificial means. But in this country, the Royal Society, ever attentive

to the improvement of science, desired their late secretary Dr. MATY to make the necessary application to the Hudson's Bay Company; in consequence of which Mr. HUTCHINS, who was then in London, and going out with a commission as governor at Albany Fort, offered to undertake the experiments, and executed them in a very complete manner, as appears from the account published in the LXVth volume of the Philosophical Transactions. In the months of January and February 1775, he twice froze quicksilver at Albany Fort; and in the first of these experiments, having broken his thermometer, he found, that the metal flattened by a fall of about six inches, bore to be hammered, gave a "deadish" sound like lead, and was finely polished on the surface. As Mr. HUTCHINS adopted exactly the method of Professor BRAUN, he observed the same phenomena, encountered the same difficulties from the sticking of the quicksilver in the tube, and cracking of his thermometer, and was equally at a loss with regard to the point of congelation. Still, however, this was the fullest confirmation that M. BRAUN's Dissertations had ever yet received; and it may be considered as a prelude, by which Mr. HUTCHINS acquired the experience that enabled him to succeed so perfectly in his last most decisive and satisfactory experiments.

§ 4. The account of Mr. HUTCHINS's success at Hudson's Bay was read before the Royal Society at the commencement of the severest winter that had been known for many years in Europe. Two gentlemen of different countries embraced this opportunity to attempt the congelation of quicksilver. The first was Dr. LAMBERT BECKER, Secretary to the Batavian Society at Rotterdam, who, on the 28th of January, 1776, at eight in the morning, made an experiment to try how low he could



could reduce the thermometer by artificial cold, the temperature of the air being then  $+2^{\circ}$ . He could not bring the mercury lower than  $-94^{\circ}$ , at which point it stood immovable; and upon breaking the bulb he saw with certainty that the outer part of the quicksilver had lost its fluidity, and was thickened to the consistence of an amalgam; it fell out of the bulb in little bits, which bore to be flattened by pressure, without running into globules like the inner fluid part.

Next day, when the thermometer stood at  $+8^{\circ}$ , he repeated the experiment with all possible exactness, after M. BRAUN's manner; but could not obtain a greater descent of the mercury than to  $80^{\circ}$  under 0, and did not again break his thermometer.

The first account I saw of these experiments was in an extract from the *Altona Post*\*, in the German language, which Sir JOSEPH BANKS had the goodness to procure from Göttingen at my request; but as they are confirmed by Professor VAN SWINDEN in his *Observations on the cold of 1776*†, there can be no doubt of their authenticity; and they afford a proof that the freezing point of quicksilver cannot be lower than  $-94^{\circ}$ , as it sunk only to that degree, and yet was in part congealed.

§ 5. The other gentleman who tried the effect of this severe cold in 1776 upon mercury was Dr. ANTHONY FOTHERGILL, at Northampton, and the account of his experiment may be seen in the *Philosophical Transactions* for that year||. His

\* *Altonaer Reichspostreuter*, 1776. N<sup>o</sup> 24. Feb. 24.

† *Observations sur le froid rigoureux du Mois de Janvier, 1776*, p. 277.

|| Vol. LXVI. p. 589.

frigorific

frigorific mixture appears to have been made with the vitriolic acid; and the natural cold of the air at Northampton that day, the 30th of January, was  $+9^{\circ}$ . It is scarcely possible to determine how far he succeeded. The quicksilver of his thermometer sunk into the bulb, and it, as well as some in a phial, contracted what Dr. FOTHERGILL calls a film on the top; but unless the scale of his instrument went below  $-40^{\circ}$ , or some solid crystals were formed, such as M. BRAUN and others observed at the commencement of congelation, nothing can be collected with certainty from this experiment.

Though the cold of this hard winter was not sufficient, either in England or Holland, for the convenient performance of experiments on the congelation of quicksilver, yet in many parts of the European continent, not farther north, it was fully intense enough for that purpose. The very morning when Dr. BICKER succeeded so imperfectly at Rotterdam, the thermometer sunk to  $-22^{\circ}$  at Rudolstadt, situated a degree more to the southward; and it had been the preceding day as low as  $-18^{\circ}$  at Berlin \*.

§ 6. I find no further attempts to freeze quicksilver till the year 1781, when Mr. HUTCHINS resumed this subject with such brilliant success. The preceding experiments had done little more than prove that quicksilver might be rendered solid by cold, and shew what sort of substance it was in that state. Nothing satisfactory had been ascertained with regard to its freezing point, or the degree of a thermometer at which it ceases to be a melted and becomes a solid metal. It must not be supposed, however, that the gentlemen who were engaged

\* Beschäftigungen der Berlinischen Gesellschaft Naturforsch. Freunde. B. II. p. 575, 576.

in these researches neglected such a principal object of enquiry ; on the contrary, Professor BRAUN himself, as hath been already mentioned, took infinite pains to investigate it, but, for want of perceiving the consequences of the metal's great contraction in becoming solid, went very wide of the truth. This source of error did not escape the penetration of other philosophers, several of whom declared their opinion that the degree of cold necessary for the congelation of quicksilver could hardly be determined by freezing a thermometer filled with that fluid. But Mr. CAVENDISH and Dr. BLACK were the gentlemen who suggested an adequate method of obviating the difficulty, so as to ascertain the point in question with certainty and precision. Reasoning on the well-known fact, that a quantity of water continues at the same temperature from the moment it begins to freeze till the whole is become solid, they very justly concluded that the same would hold good with regard to quicksilver ; and Mr. CAVENDISH confirmed this inference by experiments with metals of easy fusion, in which he found a thermometer keep at the same degree all the time they were passing from a fluid to a solid state. Hence it was proposed, that a small thermometer should be placed in some quicksilver to be frozen ; which sinking pretty regularly till the congelation began, and remaining stationary till it should be complete, would thus shew the degree of cold at which this effect takes place.

It is not at all surprising, that both the above-mentioned gentlemen should have suggested the same method, without the least communication of each other's sentiments. Most discoveries follow so closely upon a certain state of advancement in every science, that, under the present general diffusion of knowledge, similar ideas must be expected to occur about the same time to such persons as are engaged in similar pursuits. When the fruit is come to a certain degree of ripeness, more than one man

man may have strength enough to shake it off. Were we sure that philosophy would continue to be regularly cultivated, perhaps it might with truth be affirmed, that the utmost efforts of genius amount only to a power of anticipating discoveries which would necessarily be made in the course of a few years by the common progress of mankind. The principles of this experiment for determining the point of congelation in mercury being already before the world, it was most probable, that the consequences to be deduced from them would not escape gentlemen of such acknowledged sagacity, whenever they might happen to apply their attention to that subject.

Though the methods proposed by Mr. CAVENDISH and Dr. BLACK were fundamentally the same, yet there was some difference in the apparatus they recommended; and as the former gentleman got *his* executed in London and sent out to Hudson's Bay, it was that which Mr. HUTCHINS employed in performing most of his experiments. A circumstantial detail of these has so recently been read before the Society, that it would be tedious for me here to recapitulate the particulars. They have not only confirmed the preceding observations relative to the solid state into which quicksilver can be brought by cold, its metalline splendor and polish when smooth, its roughness and crystallization where the surface was unconfined, its malleability, softness, and dull sound when struck; but have also clearly demonstrated, that its point of congelation is no lower than  $-40^{\circ}$ , or rather  $-39^{\circ}$ , of FAHRENHEIT's scale; that it will bear, however, to be cooled a few degrees below that point, to which it jumps up again on beginning to congeal; and that its rapid descent in a thermometer through many hundreds of degrees, when it has once past the above-mentioned limits, proceeds merely from its great contraction in the act of freezing. These

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and the other consequences deducible from Mr. HUTCHINS's experiments have been so exactly pointed out by Mr. CAVENDISH, the real author and first mover of the whole business, that nothing remains for me but to add a few supplementary remarks.

Most of the appearances which perplexed M. BRAUN in his experiments admit of such a ready solution from these of Mr. HUTCHINS, that it would be superfluous to dwell upon each particular; there is one, however, which must be mentioned, because an erroneous explanation has been given of it by a very eminent Swedish philosopher\*. Professor BRAUN observes, that by a certain kind of management he could effect the congelation of quicksilver with very weak aqua fortis. For this purpose he filled several different glasses with snow, into which he successively poured the dilute acid of nitre, and immersed his thermometer. The mercury, which would sink only to  $-148^{\circ}$  in the first glass, came in the fourth to the term of complete congelation. It is no wonder that the professor, with the ideas he entertained, should think this extraordinary; but we now clearly understand, that the cold in the first glass was sufficient to freeze part of the quicksilver, but did not last long enough to render the *whole* solid; in the second glass a further part froze; in the third still more; till at length only such a quantity was left as the fourth mixture could fully congeal.

Another phænomenon, of which M. BRAUN gives a very unsatisfactory solution, is the sinking of the quicksilver in his thermometers after they were taken out of the frigorific mixture. This, I suppose, proceeded entirely from its beginning to melt in the warmer air, and consequently subsiding to fill up vacuities in the stem or ball, in the same manner as hap-

\* Kongl. Vetensk. Acad. Handlingar vol. XXXIII. p. 119.

pened very remarkably in some of the experiments at Hudson's Bay, and in various meteorological observations to be described hereafter.

Mr. HUTCHINS mentions, in the remarks upon his ninth experiment, that the internal surface of the congealed quicksilver, after the superincumbent frigorific mixture and fluid metal had been decanted off, "was every where very rough, and of a "dull white, resembling that of a silver spoon in common "use;" likewise, that "the lump shewed very weak cohesion, "crumbling to pieces under the strokes of a hammer, and had "not the usual polish." These differences he ascribes to an effect produced, by the spirit of nitre in the mixture, upon the quicksilver, with which it came into immediate contact; but I am rather of opinion, that they were occasioned chiefly by the unconfined crystallization of the mercury in the open gallipot employed for this experiment, similar appearances having always been observed when it congealed under such circumstances.

It is here necessary to take notice, that the thermometers sent out to Mr. HUTCHINS, even those made with quicksilver, did not exactly agree together. From his *Table of Comparison* we may collect, what subsequent experiments have confirmed, that  $-40^{\circ}$  upon the two small ones with ivory scales, answered to  $-44^{\circ}$  or  $-45^{\circ}$  upon one of the large thermometers, that which had the ball on top, and to  $-42^{\circ}$  or  $-43^{\circ}$  upon the other. Now as this last was the instrument which Mr. HUTCHINS plunged into the quicksilver he attempted to freeze in his eighth experiment, and which sunk only to  $-40^{\circ}$ , it is no wonder that he did not then perceive any marks of congelation, the cold really wanting two or three degrees of being sufficient for that effect.

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The old spirit-thermometer called *H* was erroneous no less than  $7^{\circ}$  or  $8^{\circ}$  near the freezing point of water, which served as a kind of compensation in the lower parts of the scale, so as to make it agree tolerably well with those of mercury in the greatest cold of the frigorific mixtures.

Although in two of Mr. HUTCHINS's thermometers the quicksilver sunk exceedingly low, to  $-450^{\circ}$  or near  $-500^{\circ}$ , there is reason to believe he did not in any instance obtain the extreme term of contraction, since Professor BRAUN, in some of his last experiments, brought the mercury in one thermometer to  $-544^{\circ}$ , and in another to  $-556^{\circ}$  \*. Hence it would seem, that Mr. HUTCHINS had always some part of the quicksilver left unfrozen, or some vacuum remaining, either in the stem or the ball of his instruments; and as no objection appears against those experiments of M. BRAUN's, we must conclude, that quicksilver, in becoming solid, contracts about a 23d of its whole bulk. When the principal object in view is to determine the quantity of this contraction, it will be most expedient to perform the experiment in the least degree of cold which will permit the quicksilver to be entirely frozen, that it may not be so likely to stick fast in the tube; but care must be taken to congeal the whole of the mercury in the stem as well as that in the bulb.

Among the numerous improvements in natural knowledge which have been made within a short period of years, perhaps none tends to illustrate more phænomena of nature than the late discovery, that a considerable quantity of heat disappears when bodies pass into a state of fluidity or elastic vapour, and re-appears when they are converted back again to their original condition. This remarkable effect of such changes, I believe,

\* Nov. Comment. Petrop. tom. XI. p. 313.

was first observed at Glasgow, about twenty years ago, by Dr. BLACK and Mr. IRWIN, who endeavoured to determine its most material circumstances by various experiments. Since that time Dr. BLACK has constantly taught it in his chemical lectures; and considering the heat which disappears as still remaining in the fluid or vapour, but deprived for the time of its property of being communicated to other bodies, and thereby becoming sensible, he calls it *latent* heat, a term sufficiently expressive of his manner of conceiving the fact.

In the year 1772, the celebrated Professor WILCKE inserted, in the Transactions of the Royal Academy of Sciences at Stockholm \*, a paper professedly on the subject of *the cold produced by snow in melting*, which being written in the Swedish language is less known in this country than it deserves. He seems not at all acquainted with what Dr. BLACK had done, but speaks of it as his own discovery, originating in an accidental attempt to melt away a quantity of snow by the affusion of hot water; when he found the process go on so slowly, and so little effect produced, that he determined to investigate the cause of so unexpected an event. After a series of experiments with this view, he came to the following conclusion; that snow, in melting, constantly absorbs a certain and equal quantity of heat, which is employed entirely in giving it fluidity. To render such a property more intelligible, M. WILCKE propounds a particular theory of an elastic fluid between the particles of bodies; and he proceeds to various illustrations and deductions, all highly ingenious.

Two principal methods have been adopted to prove this loss of heat; one, by adding ice at the freezing point to a certain proportion of water at a known degree of heat, and observing how much the temperature of the mixture comes out below

\* Kongl. Vetenskaps Acad. Handlingar vol. XXXIII. p. 97.

that



that which should have resulted according to the common laws of the distribution of heat among bodies; the other, by observing how much faster water near the freezing point acquires sensible heat, than an equal quantity of ice melting under similar circumstances. It is obvious, that both these methods tend not only to prove the fact, but likewise to discover the quantity of heat so absorbed; and that the latter also, if the operation be reversed, will shew the quantity of heat evolved, when a fluid congeals or becomes solid. In this way Dr. BLACK estimates the heat in question to be equal to 140 degrees upon FAHRENHEIT'S scale; Professor WILCKE, by a great variety of experiments with different proportions of snow and water, brought it out pretty uniformly about 130; and Mr. CAVENDISH, as he hath lately informed us, finds it amount to 150, and chooses to call the process a generation of heat or production of cold\*.

As the method put in practice by Mr. HUTCHINS to settle the freezing point of quicksilver depends entirely upon this generation, re-appearance or evolution of heat, by means of which the congealing quicksilver is kept at the same temperature as long as any considerable portion of it remains fluid, I thought some account of such an interesting discovery would not here be misplaced. It now becomes an important object of attention, in examining the properties of bodies, to investigate the quantity of heat produced or lost at their melting and vaporific points. So little, however, has hitherto been done in this respect, even with those bodies that are changing from fluid to solid every day before our eyes, that it is no wonder we are yet unable to determine it in a substance which has so seldom been seen in a solid state. But from the very quick congelation of quicksilver when placed in a frigorific mixture, as shewn by its

\* Mess. LAVOISIER and DE LAPLACE, from some late experiments with their new apparatus, fix it at 135.

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rapid descent in the thermometer, and from its readiness to melt again upon an abatement of the cold, apparent in all the experiments, and particularly noticed by M. BRAUN, there is reason to believe, that the quantity of heat employed in giving it fluidity is not very considerable. When water, which has been cooled below  $+32^{\circ}$ , begins to freeze, a certain part of it, proportioned to the degree of cooling, shoots at once into ice; that is, ice continues to be formed till so much heat be evolved as is requisite to bring the whole up again to  $+32^{\circ}$ . Now I am inclined to suspect, that in several of Mr. HUTCHINS's experiments the first jump of the quicksilver down from a little below the point of mercurial congelation, depended on a similar principle of the sudden freezing of such a proportion of the mercury as corresponded to the number of degrees it had been cooled below that point; hence, if the degree to which it bore to be cooled before it began to congeal, and the contraction it suffers in congealing, were both known, its quantity of latent heat, to speak in Dr. BLACK's language, might readily be found. From a rude and vague computation of this sort, I am led to believe, it is not half that of water; and if so, quicksilver seems to differ much in this respect from other metals; for tin is said, from Mr. IRWIN's experiments, to require, in order to melt, a quantity of heat which, if set loose and rendered sensible, would raise the thermometer 500 degrees.

Besides the instruments contrived particularly to try the freezing point of quicksilver, two spirit-thermometers also were sent out to Hudson's Bay, principally with a view to some collateral circumstances of the experiment. My intention in recommending them was to discover what degree of cold the freezing mixture produced; and to obtain a more exact comparison of the relative contractions of mercury and alcohol, by

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marking their simultaneous descents on a more extended scale, or as long as both of them should continue to contract regularly. The specific gravity of the alcohol employed to make these thermometers was found to be 0,8254, in a temperature of  $58^{\circ}\frac{1}{2}$ ; and they were graduated on the principle of two fixed points, one, the real freezing point of water fixed by actual experiment; the other, the 122d degree above 0, determined by comparison with a standard mercurial thermometer. This interval being divided into 90 parts, the degrees so found were measured downward as well as upward on the scale, with a proper allowance for inequality in the bore of the tube. A subsequent experiment shewed that the freezing point had been rightly marked upon these instruments; but that, in consequence of a common fault in constructing thermometers, of not heating the contents of the tube so much as those of the ball, the point of  $122^{\circ}$  was marked on both of them lower than it ought by the space of two degrees; so that  $122^{\circ}$  on the scale indicated only 120 degrees above 0 of real heat. Any detail of the observations that were made to settle the relative contractions of quicksilver and spirits by means of these instruments would be improper at present; it is sufficient to mention, that on one of them the 29th degree below 0, and on the other the 30th, were found to correspond with  $-40^{\circ}$  of the small mercurial thermometers, or more precisely with the point that would have been  $-39^{\circ}$  upon an exact standard instrument.

The other object for which the spirit-thermometers were proposed, is more immediately connected with the congelation of mercury. All former experiments with frigorific mixtures had left us absolutely in the dark with regard to the degree of cold that was really produced. By these instruments it is now determined, that the greatest effect of a mixture of snow

snow and smoaking acid of nitre, even with the advantage of such natural cold as congealed the quicksilver exposed to it, was only to diminish the heat to such a degree as would correspond with  $-45^{\circ}$  or  $-46^{\circ}$  of a standard mercurial thermometer; and consequently that the cold obtained by FAHRENHEIT in the first experiments with such mixtures, which BOERHAAVE states at  $-40^{\circ}$ \*, cannot be exceeded but by a very few degrees. This result is the more surprising, on account of BRAUN's positive assertions, that his thermometer both of rectified spirits and essential oils descended 150 or at least 100 degrees below 0†. But, since that gentleman was strongly impressed with an opinion of the excessive cold necessary to freeze quicksilver, so much as to shew evident perplexity at finding his spirit-thermometers sink less than those of mercury, I should, from this circumstance alone, be inclined to place most confidence in the experiments made at Hudson's Bay, in which no hypothesis was adopted, and therefore no prejudice can be apprehended. Indeed Mr. HUTCHINS's observations with regard to the degree of cold generated by his freezing mixtures, are so regular, uniform, and numerous, as hardly to leave a doubt that it does not exceed  $-35^{\circ}$  or  $-36^{\circ}$  of his spirit-thermometers. And this is, of itself, a very great additional proof, that the freezing point of quicksilver cannot be much lower than Mr. HUTCHINS determines it, since the mixture was incapable of diminishing the heat more than six or seven degrees further. The advantages arising from a knowledge of the cold produced, were so apparent in these experiments at Hudson's Bay, with respect to many circumstances, both in the congelation of the quicksilver itself,

\* BOERHAAVE, *Element. Chæmiz*, tom. I. p. 164.

† Nov. Comment. Petrop. tom. XI. p. 290. 316. 317. From  $260^{\circ}$  to  $300^{\circ}$  of DE L' ISLE's scale.

and in the action of the frigorific mixtures, that I should suppose spirit-thermometers will always be employed in future, whenever any thing of this kind is attempted.

§ 7. It is not a little extraordinary, that since the death of Professor BRAUN, now near fifteen years ago, all attention to the congelation of mercury should in a manner be laid aside on the spot where it was originally discovered. A dead silence on the subject seems to have prevailed at Peterburgh till this present winter; when Dr. MAT. GUTHRIE, F. R. S. Physician to the Cadet Corps of Nobles, having heard the matter much canvassed during his late visit here, resumed the consideration of it on his return to that metropolis. The only intelligence I have yet received of Dr. GUTHRIE's experiments or conclusions, is contained in one of his letters to Dr. GARTHSHORE of this Society, who has obligingly favoured me with the following extract.

"Having found," says Dr. GUTHRIE, "in my late journey to Britain, that it remained a matter of doubt, whether mercury in its pure state, unmixed with heterogeneous matter, had ever been or was capable of being congealed; I am glad to be able, from the result of several experiments, to inform you, that the purest mercury known to the chemists is capable of congelation, and in that state will bear the hammer.

"I have done something also toward determining the point of its congelation, by determining what it is not, *viz.* 150° of REAUMUR's thermometer.

"A fine thermometer, made by NAIRNE, graduated 150° of REAUMUR, that is, 337° of FAHRENHEIT below the "freezing point" [or -305°] "sunk entirely into the bulb, while the mercury in which it was plunged remained perfectly liquid, may had not as yet grown thick and gritty, a  
"phenomenon

“ phenomenon that always precedes congelation, as I have  
“ found in my experiments; nor had there as yet been formed  
“ in the inside of the tube containing the mercury to be frozen  
“ (and the thermometer to determine the point of congelation  
“ with which I stirred it) an incrustation of the metal, another  
“ indication of approaching congelation, which ever begins on  
“ the side of the tube, and gradually increases till it has  
“ reached the center, and a solid cylinder is produced.

“ From this you may form a judgement of the impurity of  
“ the mercury which some pretend to have seen congealed with  
“ natural cold; for here  $150^{\circ}$  of REAUMUR was not found a sufficient degree of cold to freeze it, and surely no such absence  
“ of heat, or any thing near it, has ever been, or ever could  
“ be, observed on the face of the habitable globe.

“ I shall only add, that my experiments were conducted on  
“ the plan of my learned friend Dr. BLACK, and spiritus niri  
“ fumans Glauberi with snow, were employed to produce an  
“ artificial cold, while the thermometer of REAUMUR stood at  
“  $20^{\circ}$  below 0 in the open air” [that is,  $-13^{\circ}$  of FAHREN-  
HEIT’s scale.]

Though the consequences here deduced by Dr. GUTHRIE from his experiment are undoubtedly erroneous, as appears from a sufficient number of other facts, yet it is not at all surprizing that they should have seemed to him just; for the error arises from a circumstance, which could not be foreseen with certainty, and occurred in several of Mr. HUTCHINS’s experiments as well as in Dr. GUTHRIE’s. To understand this, it must be considered, that when we attempt to ascertain the freezing point of water, by keeping a thermometer immersed in it while it is changing into ice, the instrument employed for this purpose is not made of water, but of a different fluid, not subject to be peculiarly affected.

affected by that particular degree of cold. In order, therefore, to render the experiment with quicksilver perfectly analogous, it would be necessary not to make use of a mercurial thermometer; but to substitute such a one as is capable of sustaining a greater intensity of cold. For otherwise, if it should happen, from any circumstance, that the quicksilver in the thermometer should begin to freeze before that in which it is plunged, the whole experiment must evidently be fruitless, as the former would sink, perhaps, many hundreds of degrees in the instrument, by its own contraction in becoming solid; while the surrounding mercury still retained its fluidity. Now this was precisely the case in Dr. GUTHRIE's experiment; the thermometer, with which he stirred his quicksilver, congealed, it would seem from the great descent, almost entirely, though he could not perceive in the quicksilver so agitated the least appearance of change to a solid state. Thus, likewise, in several of the experiments at Hudson's Bay, the mercury in the enclosed thermometer was found to freeze before that in the cylinder. Hence it is manifest, that the continuance of fluidity in a quantity of quicksilver does not secure a thermometer of that metal immersed in it from freezing.

The cause of this phenomenon is extremely uncertain. Possibly the point of congelation may not be exactly the same in all quicksilver under all circumstances. Foreign admixtures may occasion a difference in this respect; and it does not follow, that the effect of such, in certain proportions, must necessarily be to make the mercury congeal sooner, since, in the case of the fusible metal, the melting point of tin is brought lower by the addition of two metallic substances, both of which separately require a stronger heat than it for their fusion.

But as quicksilver bears to be cooled some degrees below its freezing point, before it begins to form solid crystals, the phe-

nomenon in question may depend upon that circumstance: for if, from whatever cause, the mercury in the thermometer should begin to congeal as soon as it was cooled down to  $-39^{\circ}$  or  $-40^{\circ}$ , whilst that which surrounded it would sustain a cold of  $-43^{\circ}$  or  $-44^{\circ}$  without becoming solid; it is evident, that the whole of the former might be congealed, and yet no part of the latter, though the real freezing point of both were the same, that is, though the surrounding quicksilver as soon as it came to shoot its crystals would rise immediately to  $-39^{\circ}$ , the point at which that in the thermometer froze.

As this is undoubtedly the most obscure part of our knowledge relative to the congelation of quicksilver, I endeavoured to illustrate it by some experiments on the freezing of water. The purest water I could obtain bore to be cooled to  $+21^{\circ}$ , no less than eleven degrees below the temperature to which it instantly rose as soon as the crystals of ice shot through it. This was distilled water very recently boiled; it is a mistake, therefore, that boiling necessarily renders water not so capable of being cooled below the freezing point. In proportion as the water was less pure, it seemed to congeal the sooner; and the kind of impurity which had the most effect appeared rather to be extraneous matter diffused through the water, so as to trouble its transparency, than such as was chemically dissolved in it\*. The smallest particle of ice, also, whenever the water was below the freezing point, either added from without, or by any means formed in it, would instantly cause a crystallization, by which the whole came immediately up to  $+32^{\circ}$ . Likewise a crack in the bottom of the containing glass vessel

\* I take this to be the reason that boiling has been thought to render water incapable of being cooled below the freezing point. In most kinds of water, the application of heat occasions the precipitation of earthy substances which were before held in solution; hence the water comes to be in the state of having extraneous matter diffused through it, and therefore readily congeals.

effectually



effectually prevented the water from being cooled below the freezing point, as ice constantly formed on the bottom, perhaps in consequence of the early generation of some minute portions of it in the crack. But independently of these circumstances, neither stirring, agitation, a current of fresh air on the surface, nor the contact of any extraneous body not colder, would cause the water to shoot into ice, even after it was cooled many degrees below the freezing point, notwithstanding the repeated assertions of authors to the contrary.

How far these facts may be applicable to the above mentioned instances, where the thermometer froze before the quicksilver in which it was immersed, can scarcely be determined unless more particulars were known. They shew, however, that the congelation would not necessarily be brought on by stirring the quicksilver, as practised by Dr. GUTHRIE; and point the way to various conjectures upon this difficult phenomenon, the discussion of which must be reserved for a future opportunity.

This source of error in the method for settling the point of mercurial congelation, may easily be obviated by a small change in the apparatus. Nothing further is necessary than to employ thermometers made of alcohol, essential oils, or such other fluids as will bear the requisite cold without freezing. Probably the former of these will be found most convenient: and although the contraction of other fluids does not exactly keep pace with that of quicksilver, yet as the relative proportions can be readily determined, experiments with them may at all times be reduced to the mercurial standard, being not only the most familiar, but likewise that which seems to correspond best with equal increments and decrements of heat.

There is one way also in which mercurial thermometers may be employed to ascertain the freezing point of quicksilver; I mean  
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by plunging them into some of that metal which has been frozen and is now melting. This was put in practice very successfully by Mr. HUTCHINS in his ninth and tenth experiments. It answers to the method of determining the common point of congelation upon a thermometer by melting ice, well known to be more steady and certain in its temperature than freezing water. If, however, the point of mercurial congelation be not exactly the same in different portions of the metal, it is evident, that no reliance could be placed on such an experiment; and it can scarcely be executed but with the greatest advantage of natural cold.

As Dr. GUTHRIE was mistaken in supposing he had proved that quicksilver did not congeal till it was cooled under  $-305^{\circ}$ , his suggestion of impurity in the mercury employed by others is clearly without foundation. The instances to which he refers, when that metal froze by the natural cold of the air, are rendered certain and unexceptionable, from a great variety of concomitant circumstances, confirmed by the most credible testimony, as shall presently be shewn\*.

§ 8. This account of mercurial congelation by artificial means would remain incomplete, were I not to mention that at Hampstead, on the 26th of February last, the temperature of the air being then above  $+20^{\circ}$ , Mr. CAVENDISH, by an ingenious artifice of diluting the nitrous acid to a proper degree,

\* Since this was written, Dr. GUTHRIE has sent a more perfect account of his experiments. They agree in the main with Mr. HUTCHINS's, and the difficulties which occurred to him may be solved on the same principles. It seems not improbable, that the thermometer with which the Doctor stirred his quicksilver had, by some accident, in the course of a long experiment, come into contact with the frigorific mixture, and so been set freezing.

funk the quicksilver in his thermometer to  $110^{\circ}$ , and consequently froze it in part. He then interrupted the experiment to try the cold of his frigorific mixture by a spirit thermometer, and found it nearly as great as Mr. HUTCHINS had ever produced at Hudson's Bay, that is, about equal to  $-45^{\circ}$  of a standard mercurial thermometer.

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## P A R T II.

NO other experiments have been instituted, as far as hath come to my knowledge, for the purpose of rendering quicksilver solid by frigorific mixtures; therefore I now proceed to a new series of facts, which serve partly to confirm the former, and partly to shew their application. Though the congelation of mercury, abstractedly considered, must be allowed to form a very curious and important epocha in the history of that metal, yet it is as having a reference to thermometers, by teaching us what dependence can be placed upon those instruments, fixing our ideas with regard to the different diminutions of heat, and enabling us to form a juster estimate of climates, that it chiefly becomes interesting to the human race. The subsequent part of this narrative will demonstrate, that quicksilver has very frequently become solid by natural cold; that in a few instances the effect was so palpable and obvious as to strike with immediate conviction; but that in most it has never been even suspected till the present time, the strange appearances which often occurred being imputed by the observers to

any other rather than the real cause, though they are now found to carry with them a force of internal evidence which establishes the truth beyond all doubt.

In enumerating these facts, I shall continue to pursue a chronological order. They are in general of such a kind as could scarcely become an object of attention, till thermometers had acquired some degree of accuracy. This did not happen till near the year 1730, and the first observations which prove the freezing of quicksilver were made within four or five years of that period: so intimately are improvements in philosophy connected with the perfection of instruments!

§ 1. When the Empress ANNA IWANOVNA had ascended the throne of Russia, she resolved to carry into execution one of the favourite ideas of her illustrious uncle, PETER the Great, by sending out proper persons to explore and describe the different parts of her vast dominions, and enquire into the communication between Asia and America. Three professors of the Imperial Academy were chosen for this expedition; Dr. JOHN GEORGE GMELIN, in the department of Natural History and Chemistry; M. GERARD FREDERIC MULLER, as general Historiographer; and M. LOUIS DE L'ISLE DE LA CROYERE, for the department of Astronomy; draughtsmen and other proper assistants were appointed to attend them. In the summer of the year 1733 they departed from Petersburg; and though a principal object of their commission was unavoidably neglected, from the difficulty of transporting the necessary supplies of provisions to Kamchatka, yet it was the tenth year of their travels before the survivors returned to Europe.

The thermometrical observations made in the course of this memorable survey of the Russian empire were communicated

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to the world by Professor GMELIN. His first account of them appeared in the preface to his *Flora Sibirica* \*, where a few of the most remarkable are adduced as proofs of the excessive rigour of the Siberian climate; but they were afterwards given at full length, with a more satisfactory detail of circumstances, in M. GMELIN's journal of his travels †, published by himself some years after his return. An abstract of them was also inserted in the Petersburg Commentaries for the years 1756 ‡ and 1765 §, taken, after the professor's death, from his original dispatches, in possession of the Imperial Academy. All the accounts agree together tolerably well; but as the journal is more immediately from the author, in his native language the German, and commonly contains most particulars, I thought it right to adhere principally to that work in the following narration of the facts ||.

It was at Yeniseisk, lat.  $58^{\circ}\frac{1}{2}$  N. and long.  $92^{\circ}$  E. of Greenwich, that M. GMELIN first observed such a descent of his thermometer as, we now know, indicated the mercury to have been frozen. This happened in the winter of 1734 and 1735. “Here,” says the professor ¶, “we first experienced the truth of what various travellers have related, with respect to the extreme cold of Siberia; for, about the middle of December, such severe weather set in, as, we are certain, had never been known in our time at Petersburg. The air seemed as if it were frozen, with the appearance of a fog, which did not

\* P. lxxi—lxxiii.

† Reise durch Sibirien.

‡ Nov. Comment. Petrop. tom. VI. p. 425.

§ Ibid. tom. XI. p. 320.

|| See also Mem. de l'Acad. des Sciences, 1749, p. 1.

¶ Reise, Theil. I. p. 355.

“suffer

“ suffer the smoke to ascend as it issued from the chimnies.  
“ Birds fell down out of the air as if dead, and froze immediately, unless they were brought into a warm room.  
“ Whenever the door was opened, a fog suddenly formed round it.  
“ During the day, short as it was, parhelia and haloes round the sun were frequently seen, and in the night mock moons and haloes about the moon. Finally, our thermometer, not subject to the same deception as the senses, left us no doubt of the excessive cold; for the quicksilver in it was reduced” [on the 5th of Jan. O. S.] “ to  $-120^{\circ}$  of FAHRENHEIT’s scale, lower than it had ever hitherto been observed in nature.”

Thus far Professor GMELIN. Little did he conceive that, though his thermometer was not subject to the same deception as the senses, it lay exposed to another source of error which defeated all his conclusions: for as soon as the cold became sufficiently great to produce any congelation of the quicksilver, it ceased to be a measure of the temperature; instead, therefore, of  $120^{\circ}$  below 0, the cold most probably did not exceed the point of mercurial congelation, or  $-39^{\circ}$ , but by a very few degrees, the great descent of the quicksilver, as it depended upon its contraction in the act of freezing, only affording a proof that it had really suffered this change.

We must here observe, that Dr. GMELIN’s thermometers were constructed by M. JOSEPH NICHOLAS DE L’ISLE, brother to the gentleman who went upon this expedition, on the principle invented by himself, and which still bears his name. At present such thermometers are always made by determining two fixed points, of which the uppermost, or that of boiling water, is assumed as 0, and the lowermost, or the point of melting ice, as  $150^{\circ}$ , the scale being counted downward; but in their original construction, when the utility of fixed points

was

was less understood, M. DE L'ISLE took the degrees of his scale from decrements in the bulk of the quicksilver equal to ten thousandth parts of its whole volume at the heat of boiling water. By this method the freezing point seems to have fallen about the 152d degree<sup>\*</sup>; and accordingly Professor GMELIN, whenever he has occasion to express his observations in the numbers of FAHRENHEIT's scale, reduces them on that supposition. It is not easy to discover the exact time when the present method of reckoning M. DE L'ISLE's degrees commenced; but so early as in M. BRAUN's experiments it is expressly stated, that the freezing point of his thermometer was only 150°<sup>†</sup>. There can be no doubt, however, both from theory and from WEITBRECHT's<sup>‡</sup> and GMELIN's observations, that in the thermometers used during this Siberian journey, the degree at which water congealed was nearly as low as 152°; it is according to this proportion, therefore, that I shall compute all Dr. GMELIN's observations, adhering to the common rule for such as have been made subsequent to his time with DE L'ISLE's thermometer.

The next instance of mercurial congelation to be found in GMELIN's journal exhibits a very striking example of the force of prejudice. It happened at Yakutsk, lat. 62° N. and long. 130° E. in the winter of 1736 and 1737, and is thus related by the professor §. “ This winter was unusually mild here, “ nevertheless we endured at times very severe cold, being frost- “ bitten in a sledge within the space of six minutes, notwith- “ standing all our precautions. One day, also, a certain per-

<sup>\*</sup> GMELIN's Reise. Theil. III. p. 143.

<sup>†</sup> Nov. Comment. Petrop. tom. XI. p. 299.

<sup>‡</sup> Ibid. tom. X. p. 303.

<sup>§</sup> Reise. Theil. II. p. 455.

“ son,

“son, who has some reputation in the learned world on ac-  
“count of his observations in natural philosophy, informed  
“me by a note, that the quicksilver in his barometer was fro-  
“zen. I hastened immediately to his house, to see this hitherto  
“incredible wonder of nature. Not feeling by the way the  
“same effects of cold as I had experienced at other times in  
“less distances, I began, before my arrival, to entertain suspi-  
“cions about the congelation of his quicksilver. In fact, I  
“saw that it did not continue in one column, but was divided  
“in different places as into little cylinders which appeared  
“frozen, and in some of these divisions between the quick-  
“silver I perceived an appearance like frozen moisture. It im-  
“mediately occurred to me, that the mercury might have been  
“cleaned with vinegar and salt, and not sufficiently dried. The  
“person acknowledged it had been purified in that manner.  
“This same quicksilver, taken out of the barometer and well-  
“dried, would not freeze again, though exposed to a much  
“greater degree of cold, as shewn by the thermometer. We  
“were assured by the inhabitants, that the severest cold of this  
“winter did not approach what they had suffered other years;  
“and yet the thermometer fell several times to 72° below 0 of  
“FAHRENHEIT’S scale, which would be thought, in Germany  
“at least, a very intense frost.”

The gentleman to whose observation Dr. GMELIN here shews so little respect, seems to have been no other than one of his associates in the commission, M. DE L’ISLE DE LA CROYERE\*, probably the first person upon earth who saw quicksilver reduced to a solid form by cold, and ventured to credit the testimony of his senses. As to the objection, that the same mercury did not freeze with a greater degree of cold, it is of no avail; for M.

\* See Dr. HIMSEL’s Letter, Phil. Transact., vol. LI. p. 673.



GMELIN had not any other means of estimating this but by the descent of his thermometer, which could be depended upon no farther than to the point of mercurial congelation. Nay, it is not improbable, that the more violent the cold, the less would the quicksilver appear to sink below that point, from the quicker freezing and adhesion of the small thread of mercury in the thermometrical tube. Besides, a part of the quicksilver exposed to the air might easily be frozen, and yet no appearance of such a change be perceived, if the mass did not any where separate or divide. And the fact, that it actually did freeze several times during the winter is put beyond all doubt, by the sinking of the thermometer so many degrees below the term at which that effect begins to take place. The absurd idea, that quicksilver appears to congeal in consequence of water it contains, was derived, I believe, originally from a whim of RAYMOND LULLY's. It has been the usual refuge of those gentlemen who thought proper to deny that mercury could be made solid by cold; but is too destitute of support to merit confutation.

Professor BRAUN mentions, on two different occasions \*, that the Petersburg Academy have in their possession some observations made in Siberia, which seem to shew the congelation of mercury by natural cold; but that little credit was given to them, because it had at other times been found to retain its fluidity when the cold was much more intense. Probably the observations here meant are M. DE L'ISLE DE LA CROYERE's; for that gentleman certainly transmitted papers to the Academy, according to which the mercury became solid as soon as it fell about 200° below 0 of his brother's thermometer †, cor-

\* Nov. Comment. Petrop. tom. VIII. p. 363. and tom. XI. p. 269.

† Phil. Transact. vol. LI. p. 673.

responding

responding with  $-25^{\circ}$  of FAHRENHEIT's scale. This estimation, though now found to be many degrees less than the truth, yet approaches it so near as to impress a very favourable idea both of M. DE L'ISLE's talent for observation, and of his superiority to vulgar prejudices. It is to this same gentleman that we are indebted for an account of the astonishing severity of the climate in the north-easternmost extremities of Asia. Yakutsk itself, lying further in that direction than any other place where M. GMELIN resided, evidently partakes of the same rigorous cold, if a winter in which quicksilver froze several times was esteemed unusually mild by the inhabitants.

Another set of observations, in the course of which the mercury frequently congealed, were made by Professor GMELIN at Kirenga Fort, lat.  $57\frac{1}{2}^{\circ}$  N. long.  $108^{\circ}$  E. in the winter of 1737 and 1738. His thermometer on different days stood at  $-108^{\circ}$ ,  $-86^{\circ}$ ,  $-102^{\circ}$ ,  $-113^{\circ}$ , and several intermediate degrees. Some extraordinary appearances, which very much perplexed him in these observations, not only admit of a ready solution from Mr. HUTCHINS's determination of the freezing-point of quicksilver, but also confirm it with wonderful precision.

On the 27th of November (O. S.) after the thermometer had been standing two days at  $-46^{\circ}$ , the professor found it sunk at noon to  $108^{\circ}$ . He adds\*: "I had scarcely noted down this observation, when suspecting some mistake, I ran back and examined it again. I saw the quicksilver now at  $102^{\circ}$ , and it continued rising so fast, that in the space of half an hour it had reached to  $-19^{\circ}$ ." The explanation of this phenomenon, which appeared so odd to M. GMELIN, is very evident. When the intense cold set in, the quicksilver froze in

\* Reise, Theil. II. p. 619.

his thermometer, and stuck in the tube at  $-46^{\circ}$ , that is, a few degrees below the true point of mercurial congelation. But the weather becoming milder two days afterwards, the small thread of quicksilver in the tube soon melted, and consequently subsided. Possibly it came much lower than Dr. GMELIN happened to observe it at noon; for then the great body of quicksilver was undoubtedly in motion, ascending rapidly as it expanded by melting, till it came up to the degree that corresponded with the temperature of the air. Therefore, instead of a change in that temperature from  $-46^{\circ}$  to  $-108^{\circ}$  in a few hours, and from  $-108^{\circ}$  up to  $-19^{\circ}$  in half an hour, which would have been really astonishing, this observation only shews that the cold, after having continued two days as much below  $-39^{\circ}$  as was sufficient to freeze mercury, at length abated 20 or 30 degrees, perhaps very gradually; no greater alteration than frequently takes place in most extra-tropical climates.

A similar instance occurred at Kirenga Fort a few days afterwards\*, explicable in the same manner.

Again, on the 29th of December (O. S.) Dr. GMELIN found his thermometer, which had been standing at  $-40^{\circ}$  early in the morning, sunk down to  $-100^{\circ}$  at four in the afternoon. He subjoins the following remark†. “I observed some air  
“in the thermometer, separating the quicksilver for the space  
“of about six degrees. Yesterday evening I took notice of a  
“similar appearance, except that the air was not then collected  
“into one place, but lay scattered in several. I considered it  
“as an accidental fault in the instrument, and attempted to  
“expel it by means of a steel wire, but could not bear the  
“cold. In the barometer, also, some very small air-bubbles

\* Reise. Theil. II. p. 625.

† Ibid. p. 631.

“were perceived. Next morning only a very few minute air-bubbles remained in the quicksilver of the thermometer, which had then risen to  $-44^{\circ}$ , and not the least vestige of them was to be seen in the barometer.”

It cannot be doubted, but these appearances proceeded from a congelation of the mercury in Professor GMELIN's instruments. His thermometer shewed by its descent that the cold was sufficient for this effect; and the disappearance of those supposed air-bubbles as the frost abated, demonstrates that they were nothing more than interstices formed by minute portions of congealed metal resting irregularly upon one another, and which, therefore, were gradually obliterated as the solid bits melting down united into one mass. Now this observation is of consequence, not only as proving that the quicksilver congealed, but likewise as pointing out, with great exactness, the degree of cold necessary for its congelation. For since, when only a few very minute bubbles were left, the mercury reached up to  $-44^{\circ}$  in the thermometer, its freezing point could not be below that degree, because some of it continued still solid, but must be a little higher, just so much as would answer to the expansion produced by the melting of that very small proportion of metal which remained frozen.

From Dr. GMELIN's attempt to extricate the supposed air by means of a wire, it would seem, that the tubes of his thermometers were open at top. This idea is in some measure confirmed by a passage in the preface to his *Flora Sibirica*\*, where he mentions, that upon arriving at, or quitting a place, he used to try whether his thermometer would rise to 0 in boiling water, and, if there appeared any deficiency, corrected it by the addition of fresh quicksilver; which he would scarcely have

\* P. lxxiv.

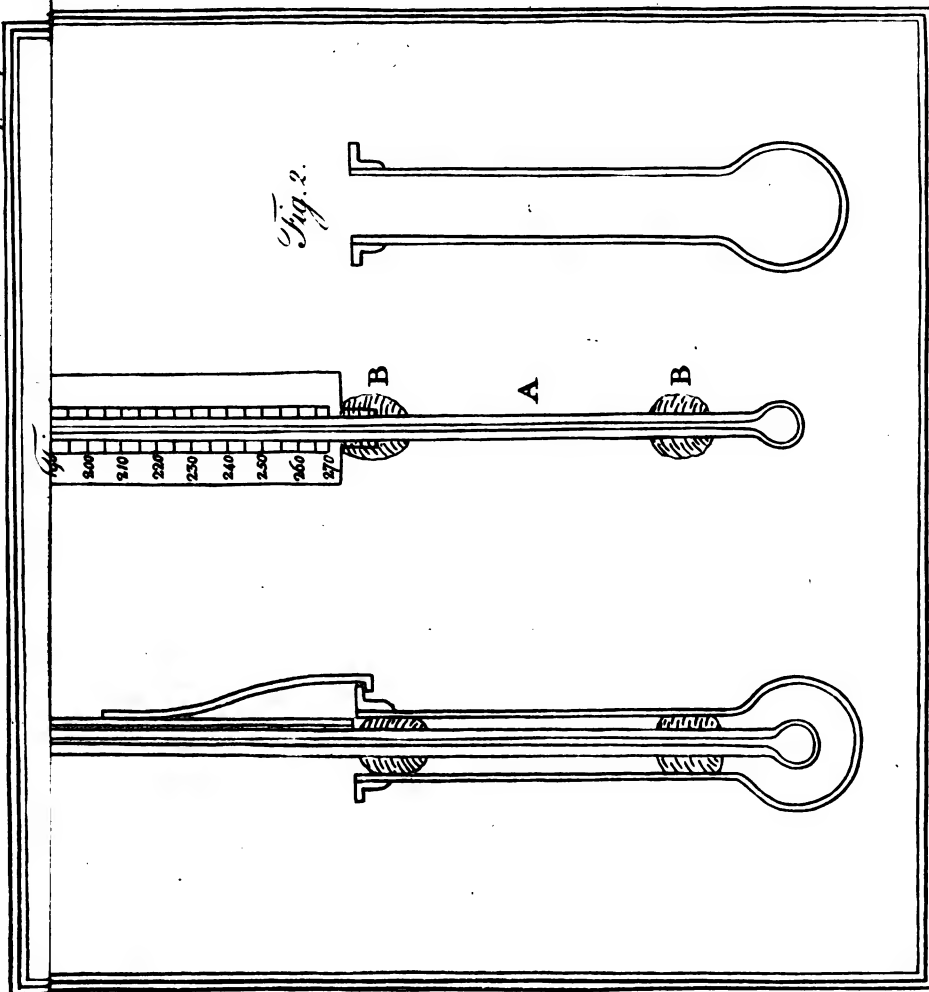
done, had the tube been sealed, on account of the great risk of spoiling the instrument in breaking it open so often. Perhaps the above-mentioned appearance of air bubbles, from the divided state of the frozen quicksilver, may have depended in part upon this exposure to the atmosphere, as well as upon the large size of M. DE L'ISLE's original thermometers. Under these circumstances we cannot suppose that the instruments were very exact.

Dr. GMELIN on several other occasions observed, that the quicksilver in his thermometer looked as if air was interspersed in it. Whenever this happened, it always subsided many degrees below what we now understand to be the point of mercurial congelation \*. The professor, totally at a loss to explain such a phænomenon, imputes it sometimes to a fundamental fault in his instrument, but which he could never discover, and at other times to an imaginary effect of the intense cold, in expelling or extricating air from the pores of the quicksilver, to be absorbed as the cold abated. On the 9th of January, 1738, O. S. the mercury sunk at once to  $-114^{\circ}$ , after having been stationary two whole days at  $-45^{\circ}$  †.

The last observations of M. GMELIN's, in which quicksilver froze, were made upon his return homeward in a part of Siberia, much nearer the confines of Europe. During the month of December, 1742, as he was passing over that branch of the Ural or Riphæan mountains which runs between Verchoturie and Solikamsk, about the 59th degree of N. lat. and scarcely 60 degrees E. of Greenwich, his thermometer sunk to  $-41^{\circ}$ ,  $-70^{\circ}$ , and at length into the bulb, though it was graduated

\* Reise. Theil. II. p. 634.

† Ibid.





to  $96^{\circ}$  below  $0^{\circ}$  \*. The same appearance of air bubbles which he had so frequently remarked in such great descents of the thermometer, puts it beyond doubt that the quicksilver was frozen. This event furnished a very striking proof of the force of habit in reconciling men to hardships, which in their common course of life are thought intolerable. Professor GMELIN, who had now been nine years in Siberia, not only bore to travel in this excessive cold, but also, in order to ascertain the height of the mountains he traversed, employed himself in observing a barometer, whilst the quicksilver was freezing in his instruments.

These are the principal of Dr. GMELIN's thermometrical observations. He collected many more, part of which were destroyed by fire or other accidents, and the remainder seem to contain no further information. They were considered by him as demonstrating the cold of Siberia to exceed that even of the most northern parts of Europe near  $100^{\circ}$  degrees, an opinion which has since been almost universally adopted; whereas we have, in fact, no proof that the difference of climate amounts to so much as the variation between one winter and another. At Yeniseisk, where the cold was so intense in 1735, it does not seem to have ever been sufficient to freeze a thermometer in the winter that M. GMELIN spent there four years afterwards; and it will soon be shewn that quicksilver has congealed more than once in Europe. All that we are authorised to conclude, therefore, with respect to the Siberian climate, is, that the cold there not unfrequently exceeds the degree indicated by  $-39^{\circ}$  of a standard mercurial thermometer.

\* Reise. Theil. IV. p. 512—515.



§ 2. This was the period of scientific enterprise. Soon after the intelligent Academicians of Petersburg had penetrated into Siberia by order of the Russian Monarch, another potent Sovereign sent out those philosophical expeditions, by which the opinion of our illustrious countryman, respecting the figure of the earth, was so honourably confirmed. As it became necessary, for the determination of this question, to measure a degree at the arctic circle, the gentlemen who undertook it were unavoidably exposed to a great severity of cold. About the time when the quicksilver was exhibited frozen to Professor GMELIN, near the extremity of Asia, without overcoming his prepossession, M. MAUPERTUIS and his associates saw the liquor congeal in their spirit-thermometer at Torneå in Lapland\*. Their mercurial thermometer sunk at the same time to  $-37^{\circ}$  of M. DE REAUMUR's scale; which, if the instrument was exactly graduated according to that philosopher's original idea, would undoubtedly shew that the quicksilver froze, as it corresponds with  $51^{\circ}$  of FAHRENHEIT. But the inaccuracies in constructing M. DE REAUMUR's thermometers have been so great, that I think no dependence can be placed upon this observation, especially as it does not appear to have been attended with any extraordinary phænomenon.

The same objection holds good with regard to the observations made by M. GAUTIER at Quebec, from the year 1743 to 1749, an extract from which is inserted in the Memoirs of the French Academy of Sciences†. The account given of his thermometer is too indefinite to allow any certain inference to

\* OUTHIER Voyage au Nord, p. 145.

† Mem. de l'Ac. des Scienc. 1744, p. 135 ; 1745, p. 194. ; 1746, p. 88. ; 1747, p. 466. ; 1750, p. 309.

be drawn; but as the quicksilver several times contracted so much as to leave a visible vacuity in the top of the bulb, and the scale seems to have reached near to its point of congelation, I am rather of opinion that it actually froze. If so, Quebec, situated in lat.  $47^{\circ}$ , is the most southern place in which such a great degree of natural cold has hitherto been observed.

§ 3. We come now to an instance of what, however often it may have happened, has hitherto never been suspected, the congelation of quicksilver in Europe by natural cold. The observations which prove this fact are recorded in the Transactions of the Royal Academy of Sciences at Stockholm, whence I have extracted the following account, from the original Swedish.

In January, 1760, the weather was remarkably cold in Lapland. On the fifth of that month different thermometers sunk to  $-76^{\circ}$ ,  $-128^{\circ}$ , or lower\*. Again, on the 23d and following days, they fell to  $-58^{\circ}$ ,  $-79^{\circ}$ ,  $-92^{\circ}$ , and below  $-238^{\circ}$  into the ball†. This great descent of the mercury was observed in four places, Torneå, Sombio, Iukasierf, and Utsioki, all situated between the 65th and 70th degrees of N. lat. and the 21st and 28th of eastern longitude, by M. ANDREW HELLANT, æconomical Inspector of Lapland, whose remarks on the phenomenon afford of themselves sufficient evidence, that the quicksilver was frozen. “During the cold weather at Sombio,” says he‡, “as it was clear sun-shine, though scarcely the whole body of the sun appeared above the low woods that terminated our horizon, I took a thermometer which was

\* Kongl. Vetensk. Acad. Handlingar. vol. XX. p. 314.

† Ibid. vol. XXI. p. 312.

‡ P. 314.

“hanging;

“ hanging before in the shade, and exposed it to the rising sun  
 “ about eleven in the forenoon, to see whether, when that lu-  
 “ minary was so low, it would produce any effect upon the  
 “ instrument. But to my great surprise, upon looking at it  
 “ about noon, I found that the mercury had entirely subsided  
 “ into the ball, though it was standing as high as  $-61^{\circ}$  at ele-  
 “ ven o'clock, and the scale reached down to  $238^{\circ}$  below 0. I  
 “ could not perceive or think, that the air had changed so sud-  
 “ denly to such an extraordinary degree of cold. I therefore  
 “ brought my thermometer, the only one I had left, within  
 “ doors, and held it before the fire, when it quickly ascended  
 “ to the usual height in a warm room. Upon being carried  
 “ out again into the open air, and placed in the shade, it sunk,  
 “ as in the forenoon, to  $-61^{\circ}$ . Afterwards I exposed it once  
 “ more to the sun-shine; but the sun having already begun to  
 “ disappear behind the horizon, the quicksilver did not subside  
 “ into the bulb as before. I then returned with the instrument  
 “ into the room, and held it in my hands before the fire, upon  
 “ which the quicksilver fell back into the ball, where it left a  
 “ vacuum or hollow bubble about the size of a pepper-corn.  
 “ When I inclined the thermometer, this bubble ran round the  
 “ ball; and after a few minutes the quicksilver rose again to  
 “ its former height.

“ I repeated these experiments several times at Nebuloslock”[a  
 settlement about ten miles distant from Sombio] “ the same af-  
 “ ternoon and the following day, by carrying the thermometer  
 “ out of the cold sometimes to the fire, and sometimes into a  
 “ warm hut; when the same thing happened, that the quick-  
 “ silver always subsided into the ball. After my return to Tor-  
 “ nea in April, I attempted to perform them again; but when  
 “ the cold was only a few degrees below the freezing point of  
 “ water,

“ water, I could never make the mercury sink, either by holding it before the fire, or carrying it into a warm room, though the experiments always succeeded when the thermometer previously stood at  $-58^{\circ}$  or lower.

“ People who were well cloathed, and in brisk motion, or driving with rein-deer, could bear this cold an hour or two; but such as travelled with horses in a sledge soon found it necessary to have recourse to the farm-houses to warm themselves. On going out from a hot room, some of the first inspirations were rather heavy and difficult; but the breathing soon became easier. It felt dreadfully cold; but still I could not perceive, from the sensation alone, that the weather was so extraordinarily severe as it appeared to be by the instruments. Having spent an hour at the house of one of my friends, in the afternoon of the 25th of January, and finding on my return that a thermometer, graduated to  $-58^{\circ}$ , had sunk into the ball, I could not at first believe it had happened from the cold, but thought the instrument must be broken, till other thermometers that were hanging near it convinced me of the truth.”

Several reflexions present themselves on the perusal of these observations. The phenomena fairly shew, that there was a sufficient degree of cold to congeal the quicksilver in Mr. HELLANT's thermometers, which sometimes sunk regularly into the bulb, but commonly stuck fast in the tube till it was heated by the sun, the fire, or a warm room, and thus made to subside. The continuance of this cold was very remarkable; it lasted no less than three days, with sufficient intensity to freeze mercury; a circumstance almost unparalleled any where, and the more extraordinary, because M. HELLANT, during twenty-three years that he had made observations in Lapland, never

before saw the thermometer so low as to indicate a congelation of the mercury. But it was not in Lapland alone that the season was uncommonly severe. At this same time the frost was nearly, if not quite, intense enough at Petersburg to freeze quicksilver, as appears from the remarks of M. BRAUN, who was then engaged in his experiments. And it is a curious coincidence of events, that on the very day when the congelation of mercury by artificial means was first clearly established in Russia, nature should be performing the same operation before the eyes of an attentive and philosophical observer in a neighbouring kingdom, who yet had not sufficient sagacity to divine her secret.

Two circumstances, however, struck M. HELLANT, which might have led him immediately to suspect the truth. The first was, that such a degree of cold as this prodigious descent of the thermometer seemed to indicate, bore no sort of proportion to the general stile of the weather in that country. What could be more incredible, than that the cold, which had never been known before to sink the thermometer below  $-40^{\circ}$ , should on one particular occasion exceed that point by hundreds of degrees, more than double the whole variation of temperature between summer and winter? Such an event would shew a want of balance in the system of nature, with respect to heat and cold, so very different from the apt adjustment of its other parts, as to be inadmissible but upon the most decisive proofs. Any reflecting person, therefore, would be more inclined to believe, that the instruments employed had ceased to be measures of the temperature, from some cause or other, than that the extremes of cold should be subject to such anomalous excesses.

The

The other circumstance of which M. HELLANT takes notice, that the sensation attending this cold by no means corresponded with its effect upon a thermometer, pointed directly to the same conclusion. When that instrument is at  $40^{\circ}$  below 0, the beams of houses crack with a loud explosion, trees split and are killed, birds fall down dead out of the air, and it is with the utmost difficulty that man, notwithstanding all his resources, can preserve the extreme parts of his body from being destroyed by the frost. Now, if the cold had been increased as far beyond this degree, as it differs from the heat of boiling water, could M. HELLANT have exposed himself to the open air with impunity? The analogy of all we know of cold declares the contrary: and though the power of animals and vegetables to resist variations of temperature has been found much greater than was formerly imagined, I think it would not be rash to affirm, that in any part of our globe where the cold was carried to such excess, the whole system of organized bodies must perish.

Yet these obvious inferences seem to have never occurred to M. HELLANT. Even the unexpected descent of his thermometer on being exposed to heat, strange and inexplicable as it must have appeared, and contradictory to all the notions he entertained, did not suggest to him a doubt of the instrument's marking the real temperature of the air. But we now know that it ceased to do so after the cold had increased a few degrees below  $-39^{\circ}$ ; that all the unusual phænomena turned upon the congelation of the quicksilver; and that the severity of this season, though greater than usual in Lapland, did not exceed that of common winters by any such remarkable difference.

The vacuum or hollow bubble, observed after the quicksilver had fallen back into the ball, shews how very much it had contracted by the congelation. This bubble moved upon inclining the

thermometer, because the external part of the frozen mass in the ball having melted before the internal, though not in sufficient quantity to fill its whole capacity, ran round it freely in a fluid state, the empty spot always rising to the top.

§ 4. Early in the spring of 1761, the Abbé CHAPPE D'AUTEROCHÉ, in his journey to Tobolsk for observing the transit of Venus, passed through Solikamsk, a town of Siberia, situated in  $59^{\circ}\frac{1}{2}$  N. lat. and  $57^{\circ}$  E. of Greenwich. On this occasion he takes notice\*, that the thermometer had sunk there the preceding winter to  $-124^{\circ}$ ; which, if the general stile of the Abbé's remarks will allow sufficient dependence to be placed upon it, would necessarily shew that the quicksilver was then frozen.

§ 5. M. ERICH LAXMANN, late Professor of Mineralogy and Chemistry at Petersburg, was resident in 1765 at Barnaul in Siberia, lat.  $53^{\circ}$  N. and long.  $81^{\circ}$  E. as minister to the German congregation of the Kolyvan Province. On the first day of that year, he saw the thermometer down so low as  $-58^{\circ}+$ ; whence it is probable, that some part at least of the quicksilver was congealed. As no concomitant circumstances are recorded with this fact, it would scarcely have been worth mentioning, were it not to introduce an account of some instruments, which became afterwards the subject of very curious observations. For M. LAXMANN, during his abode in this remote country, employed his leisure in the construction of barometers and thermometers, an art in which he acquired great skill. These he afterwards distributed, free of expence, to all

\* Voyage en Sibirie, p. 84. and 93.

† LAXMANN's Sibirische Briefe, p. 97.

parts of Siberia where they were likely to be used, with the most laudable and exemplary zeal to diffuse some rays of science through those dark and uncultivated regions\*.

All the particulars here mentioned are extracted from M. LAXMANN's *Siberian Letters*; a scarce book, because, on his return to Europe, he bought up every copy he could find, as they had been published without his consent by Professor SCHLÖZER of Gottingen.

§ 6. The benefits accruing from the travels of learned men, could not escape the penetration of the wise Empress who now reigns in Petersburg. Soon after her establishment on the throne, she ordered an expedition of the same nature as that in which Professor GMELIN had been engaged above thirty years before. Among the gentlemen who undertook this second philosophical survey of the Russian Empire, was Dr. PETER SIMON PALLAS, one of the most eminent naturalists and skilful observers of the present age. The journal of his travels is published by himself in the German language, and comprehends a rich store of curious and useful information. In general his winters were not spent in the coldest parts of Asia; twice, however, he resided at Krasnoyarsk lat.  $56^{\circ}\frac{1}{2}$  N. long.  $93^{\circ}$  E. and the last time, in 1772, had an opportunity of witnessing the most remarkable instance of the congelation of mercury by natural cold that is yet known to the world.

“The winter,” says M. PALLAS†, “set in early this year, “and was felt in December with uncommon severity. On the “6th and 7th of that month happened the greatest cold I have “ever experienced in Siberia; the air was calm at the time,

\* Sibirische Briefe, p. 29.

† Reise durch verschiedene Provinzen des Russischen Reichs, Theil. III. p. 417.



“ and seemingly thickened, so that, though the sky was in  
“ other respects clear, the sun appeared as through a fog. I  
“ had only one small thermometer left, on which the scale  
“ went no lower than  $-70^{\circ}$ ; and on the 6th in the morning I  
“ remarked that the quicksilver in it sunk into the ball, except  
“ some small columns which became solid and stuck fast in the  
“ tube. By the temperature of a room not much warmed,  
“ into which I brought the thermometer from the gallery of  
“ my house, these congealed columns immediately fell down;  
“ but it was more than half a minute before the mercury came  
“ into motion out of the ball. I repeated this experiment fre-  
“ quently, and always with similar success, sometimes one and  
“ sometimes more threads of frozen quicksilver remaining behind  
“ in the tube. When the ball of the thermometer, as it hung  
“ in the open air, was warmed by being touched with the fin-  
“ gers, the quicksilver rose; and it could plainly be seen, that  
“ the solid frozen columns stuck and resisted a good while, and  
“ were at length pushed up with a sort of violence. In the  
“ mean time I placed upon the gallery on the north side of my  
“ house about a quarter of a pound of clean and dry quick-  
“ silver in an open bowl; within an hour I found the edges  
“ and surface of it frozen solid, and some minutes afterwards  
“ the whole was condensed, by the natural cold, into a soft  
“ mass very much like tin. While the inner part was still  
“ fluid, the frozen surface exhibited a great variety of *branched*  
“ wrinkles; but in general it remained pretty smooth in freez-  
“ ing, as did also a larger quantity of quicksilver which I after-  
“ wards exposed to the cold. The congealed mercury was more  
“ flexible than lead; but upon being bent short it was found  
“ more brittle than tin, and when hammered out thin it seemed  
“ somewhat granulated. If the hammer had not been per-  
“ fectly

“fectly cooled, the quicksilver melted away under it in drops;  
“and the same thing happened when the metal was touched  
“with the finger, by which also the finger was immediately  
“benumbed. In our warm room it thawed on its surface gradually, by drops, like wax on the fire, and did not melt all  
“at once. When the frozen mass was broken to pieces in the  
“cold, the fragments adhered to one another, and to the bowl  
“in which they lay. Although the frost seemed to abate a  
“little toward night, yet the congealed quicksilver remained  
“unaltered, and the experiment with the thermometer could  
“still be repeated. On the 7th of December I had an opportunity of making the same observations all day; but some  
“hours after sun-set a north-west wind sprung up, which raised  
“the thermometer to  $-46^{\circ}$ , when the mass of quicksilver  
“began to melt.”

Before this observation of Dr. PALLAS's, no person had seen or handled quicksilver frozen by natural cold, so as to submit the fact to the public with competent evidence; but the circumstances here related are so pointed and consistent, that even those who had doubted of M. BRAUN's experiments were now staggered, and began to believe. Indeed, it was scarcely possible to suppose any mistake, when Dr. PALLAS had two whole days to repeat and vary the experiments at his leisure. But besides removing all doubts upon the congelation of quicksilver, these observations tended to shew, within certain limits, the degree of cold necessary for that effect. It was evident that the freezing point must be somewhere above  $70^{\circ}$ , because the thermometer's graduation reached only so low, and yet some part of the mercury always congealed in the tube; and as the solid masses did not begin to melt till the thermometer rose to  $-46^{\circ}$ , *that* seemed to be nearly the point at which it passes from

from a solid to a fluid state, and very possibly was so upon this instrument, a difference of several degrees being often found in thermometers so low down on the scale as  $-40^{\circ}$ , from inaccuracies in their construction.

The crystallization of quicksilver, also, became manifest on this occasion. Hence, when hammered out thin, it shewed a granulated texture. The branched wrinkles too, which formed on its surface whilst it was congealing, could scarcely have proceeded from any other cause, and suggest a general idea of the manner in which it shoots. That quicksilver should crystallize so much more visibly than most other metals, will not appear surprising, if we consider how little the cold is below its freezing point. Such substances as require, in order to melt, a degree of heat much above that of our atmosphere, experience so great a change of temperature upon being taken off the fire, that they become solid hastily, and as it were in confusion; whereas quicksilver, having never probably been exposed to a degree of cold much exceeding that of its melting point, its particles have had full leisure to arrange themselves regularly, in exact conformity to the laws of their mutual attractions. As in Professor BLUMENBACH's and Mr. HUTCHINS's experiments, so here, I imagine, some slight roughness of the surface was occasioned by this crystallization; in consequence of which M. PALLAS compared his frozen quicksilver to tin, rather than to bright silver, the appearance it always assumes when congealed in smooth glass.

Another property of quicksilver, very important to be known, was observed perhaps no where so distinctly as on this occasion at Krasnoyarsk; I mean its tendency to adhesion in freezing. Thus, Dr. PALLAS says the fragments of the congealed mass stuck to one another, and to the bowl in which they

they lay. So likewise Mr. HUTCHINS found the frozen quicksilver adhering to his cylinders and gallipot; Professor BLUMENBACH to his glass vessel; and similar facts occurred to other observers. Hence the deceptions, already so often mentioned, from the sticking of the mercury in the stems of thermometers. And this cause of error can scarcely ever fail to take place; for if quicksilver congealing in wide open vessels adheres to them wherever it touches, how can it be expected to remain loose when frozen in a narrow tube? Now, since quicksilver, under these circumstances, retains the same appearance as while fluid, from the polish given to its surface by the smooth glass, it is no wonder that such frequent mistakes have been made relative to the height of the thermometer, both in experiments with artificial cold, and in meteorological observations. At the same time it must be confessed, that such a tendency to adhere, in a metal which contracts so much in becoming solid, is not a little difficult to explain, unless we may suppose it to be the immediate effect of the crystallization.

Quicksilver, with all its other qualities of a perfect metal, seems from Dr. PALLAS's, and indeed most of the experiments, not to be completely malleable, but rather apt to break under the hammer. Perhaps it has never been sufficiently cooled to possess its metallic properties in perfection; for with respect to its melting point it may be considered as having always been hot, that is, heated near to fusion, a state in which other metals undergo a very sensible change in their properties. But when mercury congeals in vessels which confine its surface, it seems to become more malleable than under a loose crystallization.

Dr. PALLAS's travels lasted from the year 1768 to 1773, during which time *this* of the beginning of December, 1772,

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seems to be the only instance that occurred of the freezing of quicksilver. From this circumstance, and his occasional hints about the climate, I am inclined to suspect, that the cold was not so great in Siberia at this period, as when Dr. GMELIN was there from 1734 to 1742. In Europe, likewise, the winters in general appear to have been severer about the time of GMELIN's expedition than lately. It was in this very town of Krasnoyarsk that Professor GMELIN resided during the famous hard winter of 1739 and 1740; but unfortunately he has informed us of the cold there only by its grosser effects, I believe because his thermometers had been accidentally destroyed.

§ 7. Nearly 500 miles south-eastward of Krasnoyarsk is the town of Irkutsk, the capital of a Siberian Province on the vast Baikal Lake, and situated in lat.  $52^{\circ}$  N. and about the 104th degree of E. longitude. At the former of these places, the cold recorded by Dr. PALLAS began to abate on the 7th of December in the evening; and more than a day after, that is, on the 9th in the morning, it became so intense at Irkutsk as to freeze quicksilver. An account of this phenomenon, sent by Lieutenant-general VON BRILL, the governor, was published by Dr. PALLAS in his *Journal*\*, and M. GEORGI, one of the associates in this expedition, afterwards collected some further particulars†. It appears, that about four in the morning, the quicksilver was found frozen in the barometer and thermometer, its upper surface being irregularly broken. In the former of these instruments the mercury stood at 28 inches 7 lines, and the broken appearance extended through a space of about

\* Ibid. p. 419.

† *Bemerkungen einer Reise im Russischen Reich*, B. I. p. 29.

5 lines from the top downward : when it came to melt a few hours afterwards, it rose to 29 inches 7 lines, which difference of height was probably in part at least an effect of the contraction it undergoes in freezing, its greater specific gravity in the congealed state making it stand proportionably lower. In the thermometer, part of the mercury had stuck at  $-44^{\circ}$ ; and immediately under  $-59^{\circ}$  an empty space was left, equal to 11 degrees of the scale. This observation, therefore, determines almost precisely the freezing point; for none of the quicksilver could have adhered in the tube so high as  $-44^{\circ}$ , unless it had congealed before it sunk below this point, and consequently before the cold exceeded this degree. And that the mercury was really frozen became evident afterwards; for about eleven in the forenoon, as the air grew warmer, it was found to have all subsided into the bulb, the small threads in the tube melting down into the vacuity left there, out of which it did not rise again till near two hours had elapsed.

The accuracy of this thermometer at Irkutsk, apparent from its precision with respect to the point of congelation, would be matter of surprise, had we not been informed, that both it and the barometer were among the instruments constructed by M. LAXMANN during his residence in Siberia. M. GEORGI mentions them as being in the possession of Dr. WACHSMANN, the public physician of Irkutsk, by whom probably the observations were communicated to the governor.

§ 8. As the cold of America is well known to exceed that which prevails under the same latitudes in Europe, we must expect to find quicksilver freezing spontaneously in parts of that continent which do not lie very far to the northward. Accordingly, besides the instance of Quebec formerly mentioned,

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this effect takes place frequently in Hudson's Bay, even at Albany Fort, where the latitude is not one degree greater than in London. Mr. HUTCHINS, in his different situations at Hudson's Bay, has been constantly attentive to meteorological observations. During his former residence at York Fort, situated near the middle of the Bay in lat. 58 N. he was not provided with any thermometer graduated more than 70 or 90 degrees below the cypher: but he remarked, that "the quicksilver frequently sunk into the bulb," especially after having been stationary at  $-55^{\circ}$  or  $-57^{\circ}$ , and that it afterwards used to ascend to "about  $-30^{\circ}$ , indicating a greater degree of "heat than before it fell \*." These phænomena were clearly owing to its congelation, adhesion in the tube, and subsequent liquefaction as the air grew warmer. When Mr. HUTCHINS went afterwards to Albany Fort, and had procured instruments with more extensive scales, he observed the same appearances still more distinctly. His thermometers froze twice in the winter of 1774 and 1775, and three times in that of 1777 and 1778; and in every instance, except one, the mercury sunk hundreds of degrees just as the cold began to abate †. The last of these observations is rendered remarkable by the descent of the quicksilver to  $-490^{\circ}$ , the greatest ever known by natural cold, and probably very near its extreme term of contraction by freezing. In 1782, also, Mr. HUTCHINS's thermometers, together with some quicksilver in a phial, again congealed in the open air, and exhibited similar phænomena, as appears from the account of his experiments.

Fortunately in these instances of intense cold at Albany Fort, attention was paid not only to the mercurial thermometers, but

\* MS. Journal.

† Ibid.

likewise

likewise to one made of spirits, whose relative movement has been ascertained by comparison. This instrument, while the others were three, four, or almost five hundred degrees below 0, never sunk further than to a point which corresponds with  $-46^{\circ}$  of a standard mercurial thermometer. Hence it would have been easy to infer, both that the quicksilver actually congealed on these occasions, and that the degree of cold necessary for such an effect does not exceed  $-46^{\circ}$ . That the most intense cold of Hudson's Bay, during a series of several years, went so little below the point of mercurial congelation, well deserves to be noticed; and as it seems to be seldom greater in Siberia, at least in the parts visited by GMELIN or PALLAS, the effects being not more violent, perhaps we are authorized to conclude, that the extreme of artificial cold, produced by snow and nitrous acid, corresponds pretty exactly with the extreme of natural cold in the most rigorous climates which can well be inhabited.

Mr. HUTCHINS's meteorological journal at Hudson's Bay confirms what he mentions in the remarks on his third experiment, that in the coldest weather the wind is to the southward of W. which must evidently depend on some local circumstance of that country. During my own residence in America, I thought I could distinctly perceive that the coldest winds, vulgarly called *north-westers*, did not blow exactly from that quarter; but at Rhode Island, for instance, came from N.W. by W. or a point still more to the westward; at New York were rather nearer the W. than the N.; and at Philadelphia almost due N.W. On tracing lines in these directions from Albany Fort, Rhode Island, and Philadelphia, they are found to meet among the great ridges of mountains which separate Hudson's Bay from Lake Superior; whence a suspicion arises, that these mountains.



mountains may be one source of the excessive cold which particular winds occasion in North America. But another circumstance is to be taken into consideration, that when the fury of these N.W. winds abates, they very regularly draw round to the westward and southward. Now the *maximum* of cold must be, when the cold wind has blown as long as it can, and the succeeding warmer wind has not yet had time to undo any of its effect; consequently at the period when it is veering round from the northward toward the sun. Accordingly we find, that when the cold has become sufficiently great at Albany Fort to congeal quicksilver, the wind not only came from the southward of W. but commonly, also, blew with very little force; indeed the intensest frosts in all countries seem to take place with light airs or a calm.

§ 9. There are, in the possession of the Royal Society, several other meteorological registers from our different settlements on Hudson's Bay; but none of those which I have seen contain any striking extraordinary appearances, to shew that the thermometer was frozen; and the descent alone, within certain bounds, determines nothing, for these instruments were formerly constructed with so little accuracy, as to be often marked 8 or 10 degrees too low at the point of mercurial congelation. I conclude, therefore, that when Mess. WALES and DYMOND were at Prince of Wales's Fort in the winter of 1768 and 1769, the quicksilver always retained its fluidity, though it once sunk to  $-45^{\circ}$  of their thermometer\*. This Fort lies in lat.  $59^{\circ}$ ; but on account of the abovementioned ridges of mountains, and perhaps for other causes, the southern parts of the Bay may be quite as cold as the northern.

\* Phil. Transact. vol. LX. p. 153.

§ 10. We must now return to Europe. In the beginning of the year 1780, M. VON ELTERLEIN, of Vytegra, froze quicksilver by natural cold, and sent an account of his experiment in a letter to the late Professor GÜLDENSTÄDT, then at Petersburg. I obtained from Gottingen a copy of this letter, in the original German, by the friendship of Sir JOSEPH BANKS; and, translated, it is as follows:

“ On the 4th of January, 1780, the cold having increased  
“ to  $-34^{\circ}$  that evening at Vytegra, I exposed to the open air  
“ three ounces of very pure quicksilver, in a China tea-cup,  
“ covered with paper pierced full of holes. Next day, at eight  
“ in the morning, I found it solid, and looking like a piece of  
“ cast lead, with a considerable depression in the middle. On  
“ attempting to loosen it in the cup, my knife raised shavings  
“ from it as if it had been lead, which remained sticking  
“ up; and at length the whole separated from the bottom  
“ of the cup in one mass. I then took it in my hand to  
“ try if it would bend; it was like stiff glue, and broke  
“ into two pieces; but my fingers immediately lost all feeling,  
“ and could scarcely be restored in an hour and an half by rub-  
“ bing with snow. At eight o'clock a thermometer, made by  
“ M. LAXMANN of the Academy, stood at  $-57^{\circ}$ ; by half after  
“ nine it was risen to  $-40^{\circ}$ ; and then the two pieces of mer-  
“ cury, which lay in the cup, had lost so much of their hard-  
“ ness that they could no longer be broken or cut into shavings,  
“ but resembled a thick amalgam, which, though it became  
“ fluid when pressed by the fingers, immediately afterwards  
“ resumed the consistence of pap. With the thermometer at  
“  $-39^{\circ}$ , the quicksilver became fluid. The cold was never less  
“ on the 5th than  $-28^{\circ}$ , and by nine in the evening it had  
“ increased

“increased again to  $-33^{\circ}$ . In the morning the wind was “N.N.E. and afterwards N.W.”

This experiment of M. VON ELTERLEIN's deserves attention in many respects. It ascertains the freezing point of mercury with such wonderful exactness, from the melting of the solid pieces when the thermometer came up to  $-39^{\circ}$ , as to furnish a valuable corroboration of Mr. HUTCHINS's experiments, and at the same time very much to enhance our opinion of M. LAXMANN's skill in the construction of instruments. When the thermometer was thought, early in the morning, to be standing at  $-57^{\circ}$ , I imagine, that part of the quicksilver being frozen adhered in the tube. The ductility of the solid metal must have been considerable, from its yielding to the knife in the form of shavings; yet, as in most other instances, it shewed some degree of brittleness when force was applied to it in the mass. Crystallizing without impediment, it assumed an appearance which M. VON ELTERLEIN rather compares to that of lead than of silver. Its tendency to adhesion became evident from the necessity of employing an instrument to separate it from the tea-cup; and its contraction in freezing was demonstrated by the depression observed in the middle of the solid mass. This single experiment, therefore, exemplifies, in a very beautiful manner, most of the properties hitherto discovered in quicksilver, when it passes from a fluid to a solid form.

Vytegra, or Witegorfk, is situated in lat.  $61^{\circ}$  N. and long.  $36^{\circ}$  E. upon a river of the same name. It has acquired some celebrity from one of the many useful projects which occupied the active mind of Czar PETER the Great. He proposed to cut a canal from the river Vytegra which discharges itself into the Lake Onega, to the river Kovsha which joins the Belosero, or White Lake, in order to form a communication between those

two great bodies of water; but the undertaking was unfortunately interrupted by his death\*.

§ 11. The last instance I have been able to find of the congelation of quicksilver by natural cold, occurred no longer ago than the beginning of the year 1782, in Iemtland, one of the northern provinces of Sweden. M. JOHN TÖRNSTEN, Engineer-extraordinary, is the gentleman to whom we are indebted for this observation. His letter on the subject, dated from Brunflo in Iemtland, lat.  $63^{\circ}\frac{1}{2}$  N. and long.  $15^{\circ}$  E. is inserted in the Swedish Transactions for 1782†, together with some remarks upon it by Professor WILCKE.

“During twelve years,” says M. TÖRNSTEN, “that I have  
“resided here in Iemtland, the cold had never but once brought  
“the thermometer so low as  $-36^{\circ}$ , till the last day of Decem-  
“ber, 1781, when it fell in the evening to  $-54^{\circ}$ . The fol-  
“lowing new-year’s day it was sunk to  $-56^{\circ}$  at eight in the  
“morning, and by ten to  $-62^{\circ}$ . Here it continued stationary  
“several hours, but at half past four in the afternoon it was  
“observed at  $-116^{\circ}$ , and by eight the same evening it had  
“risen to  $-31^{\circ}$ . Although the quicksilver,” continues M.  
TÖRNSTEN, “thus fell to  $-116^{\circ}$  on the first of January in  
“the afternoon, I am of opinion that its descent ought not to  
“be ascribed to a proportionable increase of cold, but on the  
“contrary proceeded from the sudden change to milder wea-  
“ther, which came on that afternoon. For the preceding  
“evening, when the thermometer was standing at  $-54^{\circ}$ , I  
“remarked, that, upon bringing it into a warm room, the  
“quicksilver fell on a sudden entirely into the ball, which was

\* Büsching’s *Erdbeschreibung*, Theil I. p. 669.

† Kongl. Vetensk. Acad. *Nya Handlingar*, tom. III. p. 80.

“ about 130 degrees below 0. This experiment I repeated several times with success, but observed the following difference, that if I had not kept the thermometer in the heat long enough for the quicksilver to begin to rise again after it had sunk into the ball, it never ascended above the 130th degree by continuing in the cold, but upon being carried back into the warm room it contracted still more in the ball by a quantity which, however visible, could not be measured. On the other hand, if the instrument had been kept in the room till the mercury had risen above  $-54^{\circ}$ , it became stationary at that degree in the open air. Now, though I did not, on the 1st of January, bring the thermometer within doors before it had sunk of itself to  $-116^{\circ}$ , yet *this* fall likewise seems to have been occasioned by the change to milder weather which was then taking place. For at eight in the evening, when the external cold was at  $-31^{\circ}$ , I found that hoarfrost formed on the ball and stem of the thermometer as before, upon its being brought into a warm room; but the mercury did not sink, on the contrary it began immediately to rise.

“ Some quadrupeds perished by the intense cold, and a great number of small birds were found dead. Nevertheless, the people did not neglect going to church on this high holiday, and I have not heard that any one was frost-bitten who went out with proper cloathing.”

M. TÖRNSTEN certainly judged right when he concluded, that the fall of the thermometer to  $-116^{\circ}$  rather indicated a diminution than an increase of the cold. Though he knew nothing of the cause, yet his observation led him to a just inference, in which he displayed more sagacity than M. HELLANT on a similar occasion. All the phænomena which so much perplexed these

these gentlemen are explicable in the following manner. When the air becomes sufficiently cold to freeze quicksilver, that metal must be standing about  $-39^{\circ}$ , or, in the common way of marking the boiling point, somewhere between  $-40^{\circ}$  and  $-50^{\circ}$ , in the tube of a thermometer exposed to it. As the small thread of mercury in the tube must be more easily affected by the cold, it will probably congeal before any other part, and stick fast about the abovementioned degrees. The remainder of the mercury will then go on to freeze, and as it suffers such a great contraction in becoming solid, must leave a considerable vacuity in the bulb of any common thermometer. Consequently, when the cold, from whatever cause, comes to be less than is required for keeping the metal in a solid state, the small thread that was frozen in the tube immediately melts, and sinks down into the vacuity of the bulb, where the whole mass remains, till by its gradual liquefaction it expands again into the tube, and becomes a just measure of the temperature. This agrees exactly with what M. TÖRNSTEN observed. In the evening of the 31st the quicksilver congealed in his thermometer, and part of it stuck in the tube at  $-54^{\circ}$ , but subsided into the vacuity left in the bulb, as soon as it was exposed to heat. When the instrument had been kept in the warm room till the quicksilver re-ascended into the tube, it froze and adhered again in the open air, and the same phænomena were repeated. If M. TÖRNSTEN be exact in saying it always became fast at  $-54^{\circ}$ , the circumstance is curious, and may have depended upon some particular state of the tube in that part, or upon the first shooting of the mercury after it had been cooled to a certain degree below its freezing-point. But when the thermometer was carried back into the open air before any of the quicksilver had risen out of the bulb, the effect of the cold could not be to

force it up into the tube, and therefore no such appearances were observed as in the former case. With regard to M. TÖRNSTEN's remark, that when the whole mass of quicksilver remained in the ball it still contracted upon the application of heat, the fact is so improbable, and would be perceived with such difficulty, that I have no doubt but he was misled by some prepossession. In like manner on the 1st of January, when the thermometer, having been stationary some hours at  $-62^{\circ}$ , sunk in the afternoon to  $-116^{\circ}$ , it happened unquestionably from the melting and subsiding of a thread of frozen mercury, which had adhered in the tube of the instrument as high as the former degree. None of these effects could be produced when the thermometer had risen to  $-31^{\circ}$ , because the cold was not then sufficient to congeal the quicksilver. In this easy and simple manner, does our knowledge of the freezing point of mercury enable us to account for phenomena, which were thought so anomalous as to elude every kind of explanation. Even so lately as last year, one of the most eminent philosophers in Europe, Professor WILCKE of Stockholm, made a vain attempt to solve the difficulties by a strained application of his doctrine relative to the various specific quantities of heat in bodies, and their different attractions for the matter of heat\*.

It would now be superfluous to add, that the real cold at Brunflo was by no means what the thermometer seemed to indicate, but probably very little exceeded  $-39^{\circ}$ , or the degree of mercurial congelation, had not M. TÖRNSTEN's observations been lately represented, even in this country, as exhibiting an instance of cold actually carried to such a disproportionate and enormous excess.

\* Ibid.

Thus

Thus is the history of the congelation of quicksilver, both by natural and artificial cold, brought down to the present period. All the facts I have collected are here delivered: it is not improbable, however, that there may be others which have escaped me, especially such as are very recent, or have never been published\*; but the number already found is greater than

\* Accordingly, having gone to Paris after this paper was read, and there mentioned, at a meeting of the Academy of Sciences, our late experiments on the congelation of quicksilver, I was informed, that M. CAZALET had succeeded in rendering it solid at Bourdeaux; and soon afterwards the following account of his experiment came out in the Paris Journal, which, for obvious reasons, I shall give in the original French.

“ M. CAVENDISH, de la Société Royale de Londres, a fait dans le mois de Février dernier, l'expérience de la congélation du mercure, à Hampstead, situé à deux milles de Londres. Ce Savant est sur le point de publier son mémoire à ce sujet. Le travail du chymiste Anglois ayant été annoncé à l'Académie Royale des Sciences, dans une de ses séances, son Directeur, M. CADET DE GASSICOURT, a *revendiqué* en faveur de M. CAZALET, la congélation du mercure dans un climat beaucoup plus temperé que Londres, le chymiste François l'ayant obtenue à Bordeaux. . . . .

“ M. CAZALET, regardant l'acide nitreux concentré comme, de tous les sels, celui qui produit la dissolution de la glace avec plus de facilité, annonça en 1779, dans une de ses leçons publiques à Bordeaux, qu'il croyoit à la possibilité de la congélation du mercure, par ce moyen, dans les Provinces méridionales mêmes; mais il n'eut occasion de faire son expérience qu'au mois de Septembre de l'année dernière.

“ Il prit de la glace pilée, passée à travers un crible, la mit dans un baril dont le fond étoit un plat de porcelaine percé de plusieurs trous pour faciliter l'écoulement de la glace fondue; il plaça plusieurs tubes remplis de mercure au centre de la glace, qu'on arrosa d'esprit de nitre fumant fait par le procédé de Woulfe. On rapprochoit la glace des tubes à mesure que la dissolution s'en opéroit; il fallut 120 livres de glace pour produire la congélation du mercure. Les tubes retirés et cassés les uns après les autres, le mercure se trouva en filets comme:  
“ de:



than I expected on beginning the search. By such a connected view of the different observations and experiments in any one branch of science, we are furnished with the best opportunity of discriminating what is certain from what is doubtful, and acquire as distinct ideas as the actual state of knowledge will admit. On the present subject of mercurial congelation, the conclusions have in general been noticed, as the premises occurred. Though Mr. HUTCHINS's experiments did not stand in need of any confirmation, yet still it is pleasant to see their principal result, the freezing point of quicksilver, established by such a body of collateral evidence as, taken together, is absolutely irresistible. But besides the information obtained relative to quicksilver itself, we have been able to correct several vulgar prejudices. The difference between cold climates no longer appears so prodigious, nor the resisting powers of animals and vegetables so astonishing and inconceivable. That extensive scale of heat, which represents its diminutions by artificial means as continued down so many hundreds of degrees below the greatest produced by nature, however specious in prospect, proves to be destitute of foundation. The use of quicksilver for thermometers is at length fully ascertained. From the boiling point, to  $39^{\circ}$  or  $40^{\circ}$  below 0, it must be considered as unexceptionable, all suspicion of its irregular contraction within those

“ de l'argent. Bientôt la chaleur de l'atmosphère lui rendit sa fluidité. Cette  
 “ expérience intéressante a le double mérite d'avoir été devinée par un chimiste Fran-  
 “ çois, et exécutée par un autre; il a suppléé à ce que le climat opposoit d'obstacles,  
 “ un moyen tout-à-fait ingénieux, et qui exigeoit des connoissances en physique.” See  
 Journal de Paris, 15 Juill. 1783, p. 814.

The peculiarity of M. CAZALET's process consists in the largeness of the quantities on which he operated, and the provision he made for the useless liquor, produced by the melted ice, to run off as fast as it was formed. He certainly congealed the mercury, but did nothing to ascertain its freezing point, which he seems not even to have had in contemplation.

bounds being removed, by such a complete explanation of the cause upon which its anomalous descent in the lower part of the scale depends. On this principle there might, perhaps, be some propriety, in constructing thermometers of mercury, to fix the cypher at its point of congelation, and thence reckon the degrees of heat upwards.

The principal advantage, however, of thus passing in review all former accounts, is to furnish an important lesson to authors, which can never be too strongly inculcated, that their accuracy must be brought to the test of future discoveries. As knowledge advances, their errors, their misrepresentations, their suppression of the truth, or fictitious additions to it, shall all be infallibly detected, and heap upon their head proportionable ignominy; while the simple and candid narrative, the exact and unbiaſſed relation of facts, will acquire redoubled lustre from the fiery trial. Let every author recollect, that the day is impending, when some unforeseen improvement, affording means to sift falsehood from truth, however artfully blended, shall finally decide whether he is to be reprobated with the base herd of deceivers, or ranked among those faithful votaries of science, whose names will be delivered down with honour to posterity.

XXII. *Experiments relating to Phlogiston, and the seeming Conversion of Water into Air.* By Joseph Priestley, LL. D. F. R. S.; communicated by Sir Joseph Banks, Bart. P. R. S.

Read June 26, 1783.

TO SIR JOSEPH BANKS, BART, P. R. S.

DEAR SIR,

**A**T the persuasion of my friends, I beg you would lay before the Royal Society my late observations on *Phlogiston*, and also on the seeming *Conversion of water into air*, though I have by no means done all that I have in view with respect to these subjects. The principal *facts* are, I think, sufficiently ascertained, though I do not presume to give any opinion with respect to the *theory* of them.

I am, with the greatest respect, &c.

Birmingham, April 21, 1783.

*Expe-*

*Experiments relating to Phlogiston.*

THERE are few subjects, perhaps none, that have occasioned more perplexity to chemists than that of *phlogiston*, or, as it is sometimes called, *the principle of inflammability*. It was the great discovery of STAHL, that this principle, whatever it be, is transferrable from one substance to another, how different soever in their other properties, such as sulphur, wood, and all the metals, and therefore is the same thing in them all. But what has given an air of mystery to this subject, has been that it was imagined, that this principle, or substance, could not be exhibited except in combination with other substances, and could not be made to assume separately either a fluid or solid form. It was also asserted by some, that phlogiston was so far from adding to the weight of bodies, that the addition of it made them really lighter than they were before; on which account they chose to call it *the principle of levity*. This opinion had great patrons.

Of late it has been the opinion of many celebrated chemists, Mr. LAVOISIER among others, that the whole doctrine of phlogiston had been founded on mistake, and that in all cases in which it was thought that bodies parted with the principle of phlogiston, they in fact lost nothing, but on the contrary, acquired something; and in most cases an addition of some kind of air; that a metal, for instance, was not a combination of two things, *viz.* an earth and phlogiston, but was probably a simple substance in its metallic state; and that the calx is produced not by the loss of phlogiston, or of any thing else, but by the acquisition of air.

The arguments in favour of this opinion, especially those which are drawn from the experiments of Mr. LAVOISIER made on mercury, are so specious, that I own I was myself much inclined to adopt it. My friend Mr. KIRWAN, indeed, always held that phlogiston was the same thing with inflammable air; and he has sufficiently proved this from many experiments and observations, my own as well as those of others. I did not, however, accede to it till I discovered it by direct experiments, made with general and indeterminate views, in order to ascertain something concerning a subject which had given myself and others so much trouble.

I began with repeating the experiments in which I had found that inflammable air, made red-hot in flint glass tubes, gave them a black tinge, and was in a great measure absorbed, which I had discovered to be owing to the calx of lead in the glass attracting phlogiston from the inflammable air. As the quantity of air in these tubes was very small, though I gave it as my opinion, that the residuum in one of the processes was phlogificated air, because I perceived no marks of ascension on presenting to it the flame of a small candle; I was not, on recollection, satisfied with this conclusion, and was desirous of repeating the experiment with more care, especially as, in one of the above-mentioned experiments, I found only a very small bubble of the inflammable air in the tube in which it had been heated.

I found, however, great difficulties in repeating these experiments; and the quantity of inflammable air operated upon in them is necessarily so small, that the result is always liable to much uncertainty. I thought, therefore, that throwing the focus of a burning lens upon a quantity of pounded flint glass, surrounded with inflammable air, or rather  
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on the calx of lead alone, in the same circumstances, would be a much easier experiment, and might bring me nearer to my object; and on making the experiment it immediately answered far beyond my expectation.

For this purpose, I put upon a piece of a broken crucible (which could yield no air) a quantity of minium, out of which all air had been extracted; and placing it upon a convenient stand, introduced it into a large receiver, filled with inflammable air, confined by water. As soon as the minium was dry, by means of the heat thrown upon it, I observed that it became black, and then ran in the form of perfect lead, at the same time that the air diminished at a great rate, the water ascending within the receiver. I viewed this process with the most eager and pleasing expectation of the result, having at that time no fixed opinion on the subject; and therefore I could not tell, except by actual trial, whether the air was decomposing in the process, so that some other kind of air would be left, or whether it would be absorbed *in toto*. The former I thought the more probable, as if there was any such thing as phlogiston, inflammable air, I imagined, consisted of it, and something else. However, I was then satisfied that it would be in my power to determine, in a very satisfactory manner, whether the phlogiston in inflammable air had any *base* or not, and if it had, what that base was. For seeing the metal to be actually revived, and that in a considerable quantity, at the same time that the air was diminished, I could not doubt but that the calx was actually imbibing something from the air; and from its effects in making the calx into metal, it could be no other than that to which chemists had unanimously given the name of *phlogiston*.

Before this first experiment was concluded, I perceived, that if the phlogiston in inflammable air had any base, it must be very inconsiderable: for the process went on till there was no more room to operate without endangering the receiver; and examining, with much anxiety, the air that remained, I found that it could not be distinguished from that in which I began the experiment, which was air extracted from iron by oil of vitriol. I was therefore pretty well satisfied that this inflammable air could not contain any thing besides phlogiston; for at that time I reduced about 45 ounce measures of the air to five.

In order to ascertain a fact of so much importance with the greatest care, I afterwards carefully expelled from a quantity of minium all the phlogiston, and every thing else that could have assumed the form of air, by giving it a red heat when mixed with spirit of nitre; and immediately using it in the manner mentioned above, I reduced 101 ounce measures of inflammable air to two. To judge of its degree of inflammability, I presented the flame of a small candle to the mouth of a phial filled with it, and observed, that it made thirteen separate explosions, though weak ones (stopping the mouth of the phial with my finger after each explosion), when fresh made inflammable air, in the same circumstances, made only fourteen explosions, though stronger ones.

After this experiment I could not hesitate to conclude, that this inflammable air went totally, and without decomposition, into the lead which I formed at that time; and if the necessary circumstances of the experiment be considered, it will be thought extraordinary that, even admitting this, the result should be so decisively clear in favour of it: for, in the first place, the greatest care must be used to expel all air from the minium,

and it must be used before it can have attracted any from the atmosphere; and in the next place, the water also (a considerable quantity of which must be used, and which will also be heated in the process) should be made as free from air as possible. In these circumstances, had I found the small residuum, of 2 ounce measures from 101, to have been phlogisticated or fixed air, I should not have been disappointed; and it would not have prevented my concluding that *phlogiston* was the same thing with *inflammable air*, contained in a combined state in metals, just as fixed air is contained in chalk and other calcareous substances; both being equally capable of being expelled again in the form of air.

Afterwards using a calx of lead, which had been prepared in the same manner with the former, but which had remained some weeks exposed to the air, I found, that when by using it I had reduced 150 ounce measures of inflammable air to 10, this residuum was phlogisticated air. But examining this calx separately, I found that it gave, by heat in a glass vessel, a considerable quantity of phlogisticated air.

I must observe, that the minium should not be reduced to a perfectly compact *glass of lead*; for then it would be too refractory to be easily revived by this process. Making use of some of it, I found that I could only melt it; but that a copious black fume came from it, and coated the inside of the receiver: an experiment which I shall repeat and re-consider. I must also observe, that the lead which I procured in the above-mentioned process was not to be distinguished from any other lead, and that the inflammable air was all procured from iron by oil of vitriol.

When I made use of inflammable air from wood, I found, that though I was able to reduce minium with it, it was  
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effected with more time and difficulty. Forty ounce measures of this kind of inflammable air I reduced to 25; when I found that the heat of the lens produced only *glass of lead*, and no *metal*. The air was still, however, inflammable; and there was a small mixture of fixed air in it. This kind of inflammable air, which burns with a lambent flame, I have some reason to think, consists of an intimate union of fixed air with that which is of the *explosive* kind extracted from metals. The result of those experiments which I made with that kind of inflammable air which is collected in the process for making phosphorus, and which burns with a lambent yellow flame, was similar to those which I made with inflammable air from wood, which burns with a lambent white flame.

Having had this remarkable result with inflammable air, I immediately tried all the other kinds of air in the same manner; but in none of them did I procure any thing from the minium besides glass of lead, except in alkaline air, and vitriolic acid air. In fixed air, nitrous air, phlogisticated air, marine acid air, fluor acid air, as also in common and dephlogisticated air, I got no metal at all. In vitriolic acid air there was but a small quantity of lead produced, and I have observed that this kind of air imparts a certain portion of phlogiston to common air, rendering it in some measure phlogisticated, though by no means in so great a degree as nitrous air. Though nitrous air and phlogisticated air certainly contain phlogiston, they appear by these experiments to hold it too obstinately to part with it to minium in this process, though nitrous air quits it so readily to respirable air. I would observe, that there were some peculiar appearances in the experiments I made to revive the calx of lead in these kinds of air in which the attempt did not

not succeed; but I must repeat the experiments, and note the appearances more accurately, before I report them.

In alkaline air lead seems to be formed from the minium as readily as in inflammable air, and indeed I thought rather more so; and this is a remarkable confirmation and illustration of these experiments, in which, by taking the electric spark in a quantity of alkaline air, I converted it into three times as much pure inflammable air; an experiment which, on account of the extraordinary nature of it, I have repeated many times since I first published the account of it, and always with the same result.

This experiment also throws some light upon those in which, by super-phlogisticating iron with nitrous air, I produced a strong smell of volatile alkali; an experiment which I have also frequently repeated with the same result. The reviving of lead in alkaline air may also help us to conceive how all *acids* should have an affinity both to *phlogiston* and to *alkalies*, which have hitherto appeared to be things so very different from each other; since, from these experiments, it is probable that one of them is some modification of the other, or a combination of something else with the other. To trace the connection between the alkaline and inflammable principles, is a curious subject; and from these hints it may, perhaps, not be very difficult to prosecute it to advantage. It is evident, however, from the following experiments, that alkaline air is the compound and inflammable air, or phlogiston, the more simple substance of the two.

From  $5\frac{1}{2}$  ounce measures of alkaline air I got, by means of litharge, 17 grains of lead, besides some that was dissolved in the mercury, by which the air was confined. There remained  $2\frac{1}{2}$  ounce measures, which appeared to be phlogisticated air,  
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and to have no fixed air in it. At another time, in eight ounce measures of alkaline air I got 15 grains of lead, besides what was dissolved in the mercury, which seemed to be a good deal in proportion to it. It was observable, that there remained in this process  $3\frac{1}{2}$  ounce measures of phlogisticated without any mixture of fixed air in it, though the massicot which I used at this time gave by heat only a good deal of pretty pure fixed air. These experiments with alkaline air well deserve to be resumed, and I shall not fail to do it at a proper opportunity.

Having thus produced lead in inflammable air, I proceeded in my attempts to revive other metals from their calces by the same means; and I succeeded very well with tin, bismuth, and silver; tolerably well with copper, iron, and regulus of cobalt; but not at all with regulus of antimony, regulus of arsenic, zinc, or the metal of manganese.

I was desirous also of ascertaining by this means the *quantity* of phlogiston that enters into the composition of the several metals; but in this I found more difficulty than I had expected: and this arose chiefly from the allowance that was to be made for the inflammable air which entered into that part of the calx which was only partially revived; and it was not easy to revive the whole of any quantity of calx completely.

After many trials, I think I may venture to say, that an ounce of *lead* absorbs 100 ounce measures of inflammable air, or perhaps something more; for in one result it seemed to have imbibed in the proportion of 108 ounce measures.

An ounce of *tin* absorbs inflammable air in the proportion of 377 ounce measures to the ounce. An ounce of copper from verditer absorbed 403 ounce measures; from a solution of blue vitriol, precipitated by salt of tartar, and afterwards made red-hot with spirit of nitre, 640; but from blue vitriol itself 909

ounce measures. In this case, however, much of the inflammable air went to the formation of the vitriolic acid air, the smell of which was very perceivable in the course of the experiment. The copper that I made in this way was brittle, and therefore seemed not to be perfectly metalized; but being fused with borax it became perfect copper, and, as I think, without any loss of weight.

*Bismuth* absorbed inflammable air in the proportion of 185 ounce measures to the ounce. The calx I used was a precipitate from the solution of this metal in spirit of nitre.

*Iron* I got from a precipitate of a solution of green vitriol by salt of tartar, moistened with spirit of nitre, and exposed to a red heat. This calx absorbed in the proportion of 890 ounce measures of the inflammable air to an ounce of iron, which was in the form of a black powder; but to all appearance as much attracted by the magnet as iron filings. But it could not be expected, that perfect iron, containing its full proportion of phlogiston, should be produced in this manner, since inflammable air may be expelled from perfect iron in this very process.

*Silver* I evidently revived from a solution of it in spirit of nitre precipitated by salt of tartar, and also from *luna cornea*. A quantity of this last substance absorbed 23 ounce measures of inflammable air; but I could not get any calx of silver free from small grains of the perfect metal, which was easily discovered by a magnifier, and therefore I could not ascertain the quantity of inflammable air absorbed by it.

Small grains of regulus *cobalt* I produced from zaffre, and inflammable air was absorbed; but I did not estimate the quantity.

A quantity of *manganese* absorbed 7 ounce measures of inflammable air; but I could not perceive any thing in it which had the appearance of metal. But I imagined I had not heat enough for the purpose, and mixing with it some calcined borax, I repeated the experiment, when there was again an evident absorption of air, and in the course of that experiment, I once thought that I did perceive a small globule of metal.

*Zinc* and *arsenic* were only sublimed in this process. The same was the case with the glass of *antimony*; but the experiment was attended with this peculiar circumstance, that when the glass was melted in inflammable air it formed itself into needle-like crystals arranged in a very curious manner, though I could not produce that appearance in other kinds of air.

Inflammable air being clearly imbibed by the calces of metals, and thereby reviving them, is a sufficient proof of its containing what has been called phlogiston; and its being absorbed by them *in toto*, without decomposition, is a proof of its being nothing besides *phlogiston in the form of air*, unless there should be something solid deposited from it at the same time that the proper phlogistic part of it was absorbed. With respect to this, I can only say that, in the course of the experiments, I did not perceive any thing of the kind: for though in some of the processes there was a black smoke produced, in others I could perceive nothing but part of the calx subliming, and clouding the glass. On this account, however, I could not pretend to ascertain the weight of the inflammable air in the calx, so as to prove that it had acquired an addition of weight by being metallized, which I often attempted. But were it possible to procure a perfect calx, no part of which should be sublimed and dispersed, by the heat necessarily to be made use of in the process, I should not doubt but that the quantity

quantity of inflammable air imbibed by it would sufficiently add to its weight.

Besides the formation of metals from their calces, I had other proofs, and of a nature sufficiently curious, of inflammable air containing phlogiston, though perhaps not sufficiently conclusive with respect to its being wholly and simply phlogiston itself. Thus, by means of it, I was able to make *phosphorus*, *nitrous air*, *liver of sulphur*, and *sulphur* itself, in all of which phlogiston is acknowledged to be a principal ingredient.

Throwing the focus of the lens upon a quantity of that glassy matter which is made from calcined bones by oil of vitriol in inflammable air, some of it was absorbed, and all the inside of the receiver was covered with an orange-coloured substance, which had a strong smell of phosphorus. I then wanted sun-shine to continue the experiment; but I was satisfied that there was sufficient proof of phosphorus being actually formed in this manner. With alkaline air I succeeded much better.

In 2½ ounce measures of this air I produced, from the glassy matter mentioned above, 2 grains of phosphorus in one mass, the vessel being only filled with white fumes during the process. One-fourth of the bulk of the air remained, and this was inflammable, burning with a yellow lambent flame, exactly like that which is produced in the process for making phosphorus.

That nitrous air contains phlogiston is sufficiently evident, if there be any such thing as phlogiston: and I have farther proved, that it contains as much phlogiston, in proportion to its bulk, as inflammable air itself. I have now, however, the farther satisfaction to be able to make nitrous air from its two constituent principles, *viz.* nitrous vapour and inflammable air. The most easy process for this purpose is, to throw a

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stream of nitrous vapour into a large phial previously filled with inflammable air. In this manner nitrous air is instantly formed, and in great quantities; but as this nitrous vapour is produced by the rapid solution of bismuth in spirit of nitre, which at the same time produces a quantity of nitrous air, the experiment is not quite unexceptionable. I therefore attempted the same thing in the following manner.

Taking a quantity of what I have called a *nitrated calx* of lead, which I first produced by uniting nitrous vapour to minium (in consequence of which, from being a red and powdery substance, it became white, compact, and brittle), I placed it upon a stand, in a receiver filled with inflammable air, and throwing the focus of the lens upon it, there was a diminution of the inflammable air, which amounted to about two-thirds of the whole, and during this time lead was revived from the calx. After this there was no more diminution of the air, or revival of the calx: and then examining what remained of the air, I found it to be all strongly nitrous: and, from the circumstances in which it was produced, it must have been formed from the nitrous vapour contained in the calx, and the inflammable air in the receiver. In order to ascertain the purity of this nitrous air, I mixed it with an equal quantity of common air, and found that they occupied the space of 1,32 measures. Fresh nitrous air, made in the usual way, and mixed with common air in the same proportion, occupied the space of 1,26. This difference arose not from any impurity in the nitrous air, but from the mixture of the dephlogisticated air, which is also expelled from this calx by heat.

Liver of sulphur was procured by throwing the focus of the lens upon vitriolated tartar in inflammable air, and it appeared to be perfectly well formed.

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Lastly, to produce *sulphur*, I threw the focus of the lens on a quantity of oil of vitriol, contained in an hollow earthen vessel, and evaporated it to dryness in a receiver filled with inflammable air, in consequence of which the inside of the receiver acquired a whitish incrustation, which when warmed had a strong smell of sulphur; and repeating the process in the same receiver, I was able, this second time, to scrape off enough of the matter to put on a piece of hot iron, and to produce the genuine blue flame, as well as the peculiar smell of sulphur.

I shall conclude these observations on phlogiston with two articles; one of which seems to contradict an established maxim among chemists; and the other a former opinion of my own.

It is generally said, that charcoal is indestructible, except by a red heat in contact with air. But I find that it is perfectly destructible, or decomposed, *in vacuo*, and by the heat of a burning lens almost wholly converted into inflammable air; so that nothing remains besides an exceedingly small quantity of white ashes, which are seldom visible, except when, in very small particles, they happen to cross the sun-beam, as they fly about within the receiver. It would be impossible to collect or weigh them; but, according to appearance, the ashes thus produced from many pounds of wood could not be supposed to weigh a grain. The great weight of ashes produced by burning wood in the open air arises from what is attracted by them from the air. The air which I get in this manner is wholly inflammable, without the least particle of fixed air in it. But, in order to this, the charcoal must be perfectly well made, or with such a heat as would expel all the fixed air which the wood contains; and it must be continued till it yield inflammable air only, which, in an earthen retort, is soon produced.

Wood,



Wood, or charcoal, is even perfectly destructible, that is, resolvable into inflammable air, in a good earthen retort, and a fire that would about melt iron. In these circumstances, after all the fixed air had come over, I have several times continued the process during a whole day, in all which time inflammable air has been produced equally, and without any appearance of a termination. Nor did I wonder at this, after seeing it wholly vanish into inflammable air *in vacuo*. A quantity of charcoal made from oak, and weighing about an ounce, generally gave me about five ounce measures of inflammable air in twelve minutes.

The second article that I shall now mention affords an indisputable proof of the generation of fixed air from dephlogisticated air and phlogiston, or inflammable air. I have several times given it as my opinion, that fixed air is a *fartitious substance*, and a modification of the nitrous and vitriolic acids, my former experiments greatly favouring that conclusion; but that it was composed of dephlogisticated air and phlogiston, though maintained by my friend Mr. KIRWAN, I was far from being satisfied with, till I was forced to consent to his proof of it from my own former experiments, and gave him leave to mention it, as he has done in his late excellent paper on salts. But I have lately had two direct proofs of it by experiment.

The first was when, in repeating a beautiful experiment first made by Dr. INGEN-HOUZ, but with some variation, I was firing some shavings of iron in dephlogisticated air confined by mercury, by means of a burning lens. In this way I quickly fired the iron, and it burned away in a very pleasing manner. But what struck me most was, that, of the air that remained, a considerable portion was fixed air, though in the receiver I had nothing but the purest dephlogisticated air, together with the

iron, which could only give inflammable air. I would observe, that the melted iron formed itself into large balls, which appeared to be a mere *slag* or *glass*, and was no longer iron.

Afterwards, to put this hypothesis concerning the constituent principles of fixed air to a more direct proof, I mixed iron filings, which gave only inflammable air, with red precipitate, which I found to give nothing but the purest dephlogisticated; and when I heated them in a coated glass retort, they gave a great quantity of fixed air, in some portions of which nineteen-twentieths were absorbed by lime-water; but the residuum was inflammable. However, when I mixed with iron filings a quantity of powdered charcoal, which I had found to give only inflammable air, the fixed air produced from it was so pure, that only one-fortieth part of it remained unabsorbed by water; so that this fixed air was as pure as that which is generally procured from chalk by oil of vitriol.

It appeared, in some of these experiments, that three ounce measures of dephlogisticated air go into the composition of two ounce measures of fixed air. For one ounce of this red precipitate gave 60 ounce measures of dephlogisticated air; and when mixed with two ounces of iron filings, it gave about 40 ounce measures of fixed air that were actually absorbed by water, besides a residuum that was inflammable. I had the same proportion when I used half an ounce of each of the materials. But when I used one ounce of each, I got only 20 ounce measures of fixed air, including the residuum. At other times I had different proportions with different quantities of iron filings and charcoal.

I cannot conclude these observations without taking notice, how very valuable an instrument in philosophy is a good burning lens. This must have been perceived in many of my former

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former experiments, but more especially in these. By no other means can heat be given to substances *in vacuo*, or in any other kind of air besides atmospherical; and without some method of doing this, no such experiments as these can possibly be made. I therefore congratulate all the lovers of science on the successful attempt of Mr. PARKER to execute so capital an instrument as he has done of this kind. Such spirited and generous exertions reflect honour on himself and on our country. It is only to be wished, that we could have lenses of a smaller size (*viz.* from 12 to 18 inches diameter) made tolerably cheap, so that they might be in more common use. All my experiments were made with one of 12 inches in diameter.

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*Experiments relating to the seeming Conversion of Water into Air.*

SINCE many persons have expressed a wish to be acquainted with the experiments I have lately made, which at first seemed to favour the idea of a *conversion of water into air*, but which terminated in the discovery of a fact, in my opinion, still more extraordinary, I shall submit to the Royal Society the result of the observations I have already made; though, as yet, I have by no means been able to satisfy myself so fully as I could wish with respect to some particulars connected with the subject. All the *facts* which I shall state may be depended upon; but it is probable, that different persons may draw different *conclusions* from

from them; and to mere opinions I have never shewn myself much attached.

Having formerly observed several remarkable changes in fluid substances, in consequence of long exposure to heat in glass vessels hermetically sealed (of which an account may be seen in the fourth volume of my Philosophical Observations); I then formed a design of exposing all kinds of solid substances to great heats, in a similar state of confinement; and for that purpose provided myself with a cast-iron vessel, which I could close at one end, like a digester, and of such a length, that one of the ends might be red-hot, while the other was sufficiently cool to be handled. To this end there was a cock connected to a tube, by means of which I could let off steam, or air, in any period of the process.

I imagined, that when substances consisting of parts so volatile as to fly off before they had attained any considerable degree of heat, in the usual pressure of the atmosphere, were compelled to bear great heats under a greater pressure, they might assume new forms, and undergo remarkable changes, similar to what we may suppose to be the case within the bowels of the earth, where, by means of subterraneous fires, various substances bear great heats under very great pressures.

I have had this instrument some years; but it was so ill constructed, that I could not make the use of it that I had originally intended. I therefore lately fitted up some gun-barrels in the same manner, and made my first experiment with limestone; expecting, that when the fixed air, and other volatile matters, that might be contained in it, should be compelled to bear a red heat, without a possibility of making their escape, the substance itself might undergo some change; but I had no particular expectation concerning the nature of that change.

I had, however, been so often favoured with valuable results from merely putting things into new situations, that I was encouraged to make the experiment; but I found an unexpected difficulty in getting a cock that would be air-tight and steam-tight under so great a pressure as I wished to apply.

I was mentioning these ideas to Mr. WATT, in whose neighbourhood I have the happiness to be situated, when he mentioned a similar idea of his, *viz.* that of the possibility of the conversion of water, or steam, into permanent air; saying, that some appearances in the working of his fire-engine had led him to expect this. He thought that if steam could be made red-hot, so that all its latent heat should be converted into sensible heat, either this or some other change would probably take place in its constitution. The idea was new to me, and led me to attend more particularly to my former projects of a similar nature, and I began with lime-stone, wishing to try the effect of giving a red-heat to lime in which water only should be previously combined, thinking it might possibly have the same effect with making the water itself red-hot.

Accordingly, I took a quantity of well calcined lime, and mixing with it a little water, out of which all air had been carefully boiled, I exposed it gradually to a strong heat in an earthen retort, such as I had been usually supplied with by Mr. WEDGWOOD (who is as much distinguished by his love and generous encouragement of science, as he is by his improvements in his own curious art), not imagining that it could make any difference whether the lime, so prepared, should receive its heat in an earthen retort, or in a vessel of iron or glass. Proceeding, however, in this manner, I found that nothing came over in the form of *steam*, but that there was a great quantity of *air*, several hundred times more than the bulk of the water, and at

that time there was in it a considerable proportion of fixed air, which I imagined might either be that which had not been sufficiently expelled before, or might be composed of some phlogistic matter contained in the lime, and the purer air that was yielded by the water: for I own I then concluded, that the air which I got (and which, when the fixed air was extracted from it, was such as a candle would just burn in) came from the water, especially as in some of the processes, the weight of the air I caught was very nearly, if not quite equal to that of the water, and interposing a large glass balloon between the retort and the recipient for the air, I observed that it remained perfectly cool and dry during the whole process; and several hours afterwards there was not the least moisture condensed in it. I also received a quantity of another produce of air made in this manner in mercury, and having viewed it with the greatest attention, observed that, after several days, it never deposited the least moisture.

I then calcined a quantity of natural lime-stone with this glass balloon, interposed in the same manner, and found no water, but only air to come from it, though the stone is generally supposed to contain water. But when I used much more than half an ounce of water to the quantity of whiting or lime above-mentioned, I always had some water come over, though very little in proportion to the quantity made use of.

I did not fail to examine whether there had been any loss in the weight of the lime, or whiting, in order to determine whether any part of these solid substances had entered into the composition of the air; but I found much difficulty in weighing them with exactness, after shaking them out of an earthen retort, into which I could not see, and to which part of these earthy matters often adhered, so that I could not obtain much satisfaction even when I broke the retort. Besides, there was

always some loss of the earth in the cloudiness of the air; whenever the production of it was rapid. In a future process I had abundant proof that the air did not come from any earthy matter with which the water had been combined.

Hitherto I had no idea but that all that was necessary to the conversion, as I concluded it to be, of water into air, was to give it a red heat, without which it would not quit the calcareous earth; and I imagined by this means the matter or principle of *heat* was so intimately combined with it, as not to be separated from it by cooling, as in the case of steam. But I, as well as all my friends, was a long time utterly disconcerted upon finding that when I put the whiting and water into a coated glass retort, the water came over in the form of steam, and little or no air was produced. The result was also the same when I made the process in a gun-barrel, in a porcelain retort, or even in an earthen retort glazed in the inside.

That the earth had not lost its property of doing its part in the business, I found by putting more water to the same whiting which had failed in the glass retort, and which had been used no less than four times before, and then heating it in an earthen retort; when again it gave air only, and no water, the same as before. And at this time I observed, that part of the air was hardly to be distinguished from that of the atmosphere.

I cannot express my surprise at my unexpected failure with the glass retort; and my speculations on the subject were various, but at that time altogether ineffectual. Among other things it occurred to me that, possibly, some phlogiston, either contained in the earthen retort, or coming through it (though I could not tell how, or on what principle) from the fire, might be necessary to water, and all other substances, assuming the form of air. But when, with this idea, I put spirit of wine, oil,

oil, or iron-filings to the lime, I got nothing from these mixtures in glass retorts besides steam and inflammable air, from the decomposition of these substances containing phlogiston.

That there was nothing in the materials of which the earthen retort was made that necessarily produced the air, was evident from my not succeeding when I pounded a broken retort, and heated it, mixed with water, in one of glasses.

Being satisfied that the production of air depended very much upon the retort itself, I thought of using the retort only with water, but without any lime, or earthy substance; and I found it succeed far beyond my expectation. For when I put a small quantity of water into one of these retorts, and endeavoured to distil it gently, I never failed to procure about an hundred ounce measures of air; and this I could do as often as I pleased, with the same retort, and without its losing any weight; and the air produced in this manner had never any portion of fixed air in it, and was always but very little inferior to that of the atmosphere.

In all these processes I observed, that very little of this air was procured till all the water that could be poured out of the retort was evaporated, for the difference in the produce was very little, whether I exposed the retort to the fire quite full of water, or with only about an ounce measure of water in it, or even after letting it remain full for a short time, and then pouring out all that I could from it; so that it was only that water which was entangled, as it were, in the pores of the retort, and which had been in some measure united to the substance of it, that had contributed to this production of air.

These retorts (which Mr. WEDGEWOOD informs me are made of a mixture of fresh and of burnt Devonshire pipe clay) are pervious to water, though not to air; so that while the air is produced



produced from that water which has entered the pores, the rest is sometimes visibly making its escape in the form of a copious smoke on the outside. It was evidently impossible, however, and contrary to all the laws of hydrostatics, that air should enter by the same pores by which the water or steam was escaping, and at the same time that its endeavour to force its way out of the retort was such that it overcame a considerable resistance from the column of water, at the mouth of my recipients. Air might have *escaped* through any unobserved pores in the retort, but none could have *entered* that way: and if there was the least sensible crack in any part of the retort, I was never able to collect any air at all.

But the following experiments may, perhaps, shew that it is sufficient for the production of air that steam come into contact with clay sufficiently heated. Between a copper still and the glass tube communicating with my recipient for air, I introduced the stem of a tobacco-pipe; and by means of a small furnace, I kept about three inches of the middle part of it moderately red-hot. In this state, making the water boil, I uniformly received air, though mixed with steam, at the rate of five ounce measures in twelve minutes for more than an hour; but when I let the pipe cool, nothing but steam was delivered by it without any air at all. There was no fixed air in this produce, and it was all such as a candle would hardly have burned it. It might, I thought, have been better and also more in quantity if I had not used the stem of a foul pipe. But when I used a clean pipe in the same manner, I did not find the air much, if at all improved. Suspecting this to arise from the near contact of the fuel, I inclosed the tobacco-pipe in an earthen tube, and then I had air as good as I had generally  
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got in the earthen retort, and not much worse than that of the atmosphere.

Another circumstance I observed was, that if the outside of the vessel which contained the water or steam, through which it passed, when the requisite heat was applied to it, was not dry, or perhaps surrounded with good air (for in those circumstances the following experiment differs from the preceding ones) the experiment did not succeed.

When I put the ball of an earthen retort, filled with moist clay, into an iron digester, and applied heat to it, I get only a very little fixed air, which was probably composed of a small quantity of air beginning to be produced from the materials and inflammable air from the vessel. All that come over besides was steam, and at last inflammable air, from the vessel itself.

Being now able to procure air by means of water in this most simple method, *viz.* by water only in the earthen retort, I had an opportunity of ascertaining, with great ease and exactness, several circumstances relating to the process, and of obviating, as I thought, some objections to the conclusion that I had drawn from it. Among other things I fully satisfied myself that the *earth of the retort* contributed nothing at all to this production of air, but the *water only*: for having used the same retort till I had got from it nearly an ounce weight of air, or 800 ounce measures, I found that it had not lost so much as a single grain in weight. After the first process it weighed just three grains more than it did at first, and it continued to weigh the same till after the last process. This small addition of weight might easily have come from a little of the water having been imbibed by the neck of the retort, where the heat of the fire could not reach it. When all the processes were over,  
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I kept the whole retort in a red heat for several hours, and then found that, besides losing those three grains, it weighed eight grains less than it did at first.

Before this I had found, that the calcined whiting which I had used in the first experiment could not, as some supposed, attract from the atmosphere any considerable part of the air which I got from it, after combining water with it: for two ounces of the whiting (which was the quantity which I generally made use of) did not attract more than eight grains of any thing when it was exposed a whole day in an open dish, though it had lost more than half its weight in calcination.

It has been imagined by some, that the air which I got in these earthen retorts was that which had been attracted from the atmosphere by the inside surface of them. But, besides that no air could ever be produced without water, to obviate this objection more particularly, when one of these retorts was giving its last portion of air, I immersed the mouth of it in a basin of water; and letting it cool in that situation, filled it again without admitting any access of air to the inside; and yet, on repeating the process with it, the air was produced just as freely as before. This operation I repeated several times. If it be said, that the outside of the retort attracted the air, still the inside, being composed of the same materials, must have attracted air also; and it would have appeared by the ascent of the water from the basin, the retort being sufficiently impervious to air.

By some it was imagined, that either the air itself that I procured, or at least the power of the retort to contribute to the production of it, was owing to something that was transmitted from the burning coals, but which could not pass through glass or metals. To determine this, I took an earthen  
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tube, of the same composition with the retort, and putting a little water in it, placed it, surrounded with sand, in a glass vessel, and this again, surrounded also with sand, in an iron one; and yet the heat transmitted through all these substances enabled the earthen tube to give air, in the same proportion, and of the same quality, as it would have done if it had been exposed to the naked fire.

Having now procured air, by means of a water, in a very simple and, as I thought, an unexceptionable manner, I wished to make it in greater quantities in proportion to the water employed; and for this purpose I first thought of increasing the size or the thickness of the porous retorts; but I thought it might answer as well if I put into the retort, in powder, the materials of which they were made, or other substances of the same kind.

Accordingly, by mixing ground flint and clay in various proportions, I presently increased the quantity of air much beyond my expectation. In the first trials, in which I had much flint and a little clay, I never failed to get 200 ounce measures of air from one of water. Then, using more clay and less flint, I had still more air; and at last, leaving out the flint altogether, and using clay only, I never failed to get much more than 400, and generally between 500 and 600, ounce measures of air from one of water, which was about three-fourths of the weight of the water; and in one particular process I procured very little less than nine-tenths of the weight of the water in air, and this air was never much less pure than that of the atmosphere. Sometimes it could not be distinguished at all from it at all by the test of nitrous air; and once or twice I thought it even purer than that of the atmosphere.

I must here observe, that I found it not convenient to put so much water to any quantity of clay as would make it cohere in one mass, but only so much as that it should remain in the

form of powder. By this means it would easily pour out of the retort when the experiment was over.

The weight of the water expended in this production of air I ascertained, in the most unexceptionable manner, by weighing the retort, with all its contents, before and after the process. I shall explain this by the result of two of the processes. In one of them, the retort and moistened clay together lost in weight 1 oz. 4 dwts. 12 grs. after yielding 741 ounce measures of air, which (in the proportion of six grains to one ounce measure) would have weighed 18 dwts. 12 grs. and consequently three-fourths of the weight of the water.

In the other process the loss of weight was 15 dwts. 18 grs. after yielding 556 ounce measures of air, which would have weighed 13 dwts. 21 grs. The proportion, therefore, between the weight of the air and that of the water was 111 to 116, or nearly nine to ten.

I also found now, that so much heat as I had hitherto applied was neither necessary nor useful. In the last mentioned process the retort was constantly suspended about six inches above a moderate charcoal fire; at another time more than twelve or fifteen inches above it, where a FAHRENHEIT's thermometer did not shew more than 210°. With this moderate heat I got 465 ounce measures in the course of about twelve hours. When the retort was suspended within six inches of the fire, the air was generally produced at the rate of 30 ounce measures in five minutes. But a thermometer, the bulb of which was immersed in the clay, was still only at the heat of boiling water.

In all these processes, however, there was evidently some loss of water; for, excepting the first experiment with the lime, I never got the whole weight of the water in air; and  
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it might be said that I only expelled the air before contained in the water, though from these experiments it appeared to contain much more air than it had been thought capable of containing. To obviate this objection, I contrived to catch all the water that escaped through the pores of the retort in the following manner.

Having put the moistened clay in an earthen tube, to which I had fitted a cock and a long glass tube (by means of which I could collect all the air that came from it); I put this within an iron tube, which was closed at the end next the fire, but open at the other end, and so long that I could easily keep this open and quite cool while the other was in the fire; consequently, whatever water escaped through the pores of the earthen tube, it would be condensed in the cool part of the iron one. This water I carefully collected, and always found that the weight of it, together with that of the air produced in the experiment, was nearly that of the original weight of the water, estimated by the loss of weight in the earthen tube and its contents. I also found, that the water so collected served for the production of more air, just as well as any other water whatever, so that there had been no decomposition of the water in the case.

In the laſt proceſs that I went through of this kind, the loſs of weight in the earthen tube, or rather of the water contained in it, was 12 dwts. 4 grs.; the air collected was 173 ounce meaſures, which would have weighed 4 dwts. 3 grs. and the water which eſcaped through the pores of the earthen tube, and which I collected, was nearly 8 dwts. 3 grs.; ſo that the air and this water together weighed 12 dwts, 14 grs. or ten grains more than the original water. But as I eſtimated the weight of the water only by the ſpace which it occupied in a

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cylindrical glass tube, divided according to ounces and parts of ounces of water, it was not easy to avoid an error of a few grains. At other times there was an error of a small magnitude on the other side. But it will appear hereafter, that more steam must have escaped invisibly at the open mouth of the iron tube than I was aware of.

That nothing could enter by the pores of the retort at the same time that the water was making its escape out of them, I thought I ascertained pretty satisfactorily by immersing the bulb of it in mercury, contained in an iron vessel. In these circumstances I obtained air as usual, only the produce was not so rapid. In this way, however, I procured above an hundred ounce measures of air from moistened clay; and I discontinued the process without perceiving any termination of it. But the moment the retort was raised out of the mercury, it gave air three times as fast as it had done before. The quality of the air was the same in both cases, *viz.* a little worse than that of the atmosphere.

I even collected thirty ounce measures of air when the bulb of the same retort was immersed in hot linseed oil, but the production of air gradually ceased, and the next day I found the retort almost full of the oil, which had soaked through it. Distilling this oil I get 300 ounce measures of air, wholly inflammable, except a very few ounce measures at the last, which were only phlogisticated.

Still hearing of many objections to the conversion of water into air, I now gave particular attention to an experiment of Mr. CAVENDISH's, concerning the re-conversion of air into water, by decomposing it in conjunction with inflammable air. And in the first place, in order to be sure that the water I might find in the air was really a constituent part of it

it, and not what it might have imbibed after its formation, I made a quantity of both dephlogisticated and inflammable air in such a manner as that neither of them should ever come into contact with water, receiving them as they were produced in mercury; the former from nitre, and in the middle of the process (long after the water of crystallization was come over), and the latter from perfectly-made charcoal. The two kinds of air thus produced I decomposed by firing them together by the electric explosion, and found a manifest deposition of water, and to appearance in the same quantity as if both the kinds of air had been previously confined by water.

In order to judge more accurately of the quantity of water so deposited, and to compare it with the weight of the air decomposed, I carefully weighed a piece of filtering paper, and then having wiped with it all the inside of the glass vessel in which the air had been decomposed, weighed it again, and I always found, as nearly as I could judge, the weight of the decomposed air in the moisture acquired by the paper.

As there is a source of deception in this experiment, in the small globules of mercury, which are apt to adhere to the inside of the glass vessel, and to be taken up by the paper with which it is wiped, I sometimes weighed the paper with the moisture and the mercury adhering to it; and then exposing it in a warm place, where the water would evaporate, but not the mercury, weighed it again, and still found, as nearly as I could pretend to weigh so small a matter, a loss of weight equal to that of the air. I wished, however, to have had a nicer balance for the purpose: the result was such as to afford a strong presumption that the air was re-converted into water, and therefore that the origin of it had been water.

Another



Another presumption in favour of the generation of our atmosphere from water was, that the purity of the air that I produced from it is so very nearly the same with that of the atmosphere. And the degree of heat requisite to produce it is no greater than may be given by the rays of the sun in certain circumstances. Subterraneous fires, however, would be abundantly sufficient for the purpose, as it appears to be sufficient for the conversion of water into respirable air, that it come into contact with clay, and perhaps many other earthy substances in the form of vapour. I must, however, observe, that when I threw the focus of a burning lens upon a quantity of moist clay, either *in vacuo*, or in common air, I got no air from it.

I made this experiment both with the clay exposed in an open dish, and also confined in a short earthen tube. Had I then proceeded to repeat this last process with a communication between the inside of the earthen tube and the external air, as I then proposed to do, but was prevented, I should much sooner have discovered what I did afterwards, *viz.* that there was no real conversion of water into air in this process. In favour of which, however, it may not be amiss to observe, that the great difficulty Mr. DE LUC and others have found in expelling all air from water, is best accounted for on the supposition of the generation of air from water, though in other circumstances than those that I have observed. I have the pleasure to add, that Mr. DE LUC himself concurs with me in this opinion.

The difficulty that strikes many persons the most forcibly, is the want of analogy between the conversion of water into air with any other known facts in philosophy or in nature. But admitting that this conversion is effected by the intimate union  
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of what is called the *principle of heat* with the water, it appears to me to be sufficiently analogous to other changes, or rather combinations of substances. Is not the acid of nitre, and also that of vitriol, a thing as unlike to air as water is, their properties being as remarkably different? And yet it is demonstrable, that the acid of nitre is convertible into the purest respirable air, and probably by the union of the same principle of heat.

It is true, that steam is a thing very different from air, and I find that it is not able to decompose nitrous air; but then, though it has acquired sensible heat, it has got no latent heat so intimately combined with it as it is with air; and for the same reason, perhaps, the vapour of nitrous acid is not dephlogistified air.

By the same process by which respirable air is made by means of water, inflammable air may be made from liquid substances containing phlogiston. Making spirit of wine to boil in a glass retort, I made the vapour pass through the stem of a hot tobacco-pipe, and found that it was all seemingly converted into inflammable air, and it was of that kind which burns with a lambent white flame. But when I let the pipe cool no air was produced, but only vapour, which was instantly condensed in the water.

Being now master of a new and easy process, I was willing to extend it to other liquid substances; and I presently found, as I then imagined, that, by this means, I could give a permanent ærial form to any liquid substance that had been previously thrown into the form of vapour.

When I made the vapour of spirit of nitre, heated in a glass retort, pass through the stem of the hot tobacco-pipe, I got as pure dephlogistified air as ever I have procured from

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nitre; though the cork, by which the retort was connected with the pipe, was dissolved, and must have contributed to contaminate it, and give it a slight mixture of fixed air.

With oil of vitriol I got air considerably phlogisticated, so that a candle would not have burned in it; but this I also attribute to the cork, which was dissolved in the process. The result was nearly the same when I used water impregnated with vitriolic acid air, though the cork was not dissolved. But this acid is known to contain much phlogiston.

Spirit of salt gave air no purer than the best atmospherical air. But as by this process I never got air so pure as this from water only, I concluded, that even this acid, as well as the nitrous and vitriolic, is capable of being turned into dephlogisticated air.

When I used water impregnated with fixed air, this air was expelled by the heat, and came over without any change that I could perceive, except that the residuum was larger, from the water that came along with it. The air I got afterwards was only that from the water, and of the same quality as if it had not been impregnated with fixed air.

Water impregnated with alkaline air gave neither fixed nor inflammable air, which I had rather expected, but only air considerably phlogisticated; though some of it was so pure that a candle would have burned in it.

N. B. In all these experiments with the tobacco-pipe all the air was remarkably turbid, like milk, and even the common air in the retort before the process properly began.

In this state of the experiments I think I may venture to say, that no person could have seen them without concluding that there was a real conversion of water into air, there being no known principle or fact in philosophy, that could have led

any person to suspect a fallacy in the case. In this, therefore, I must have acquiesced, as indeed did all my acquaintance, even those who had been the most incredulous on the subject, after they had themselves seen the experiments. But I was led to the farther prosecution of this business, in consequence of having observed that the *purity* of the air which I procured depended upon the state of that which was immediately contiguous to the earthen retort, or tube, in which I supposed the conversion to have been made; and that some communication with the atmosphere was necessary to the production of any air, as in the experiment with the digester, and those with the clay and the burning lens. And since pure external air was necessary in order to procure good air, it was concluded by several of my friends, and especially Mr. WATT, that the operation of the earthen retort was, to transmit phlogiston from the water contained in the clay to the external air; and that the water, thus dephlogisticated, was capable of being converted into respirable air by the intimate union of the principle of heat.

In order to ascertain what the influence of the external air in this case really was, I inclosed an earthen retort filled with moistened clay in a large glass receiver, open at both ends, through the upper orifice of which (being narrow) I thrust the neck of the retort, luting it so as to be perfectly air-tight; and placing the receiver in a basin of water, by which the air within was cut off from all communication with the external air, I fitted to the mouth of the retort a glass tube, through which I could receive whatever was produced in the process. In this situation I heated the retort by means of Mr. PARKER's excellent burning lens, when air was received through the tube communicating with the inside of the retort as usual; but at the same

time the water rose within the receiver. This effect might be owing to a phlogistication of the air within the receiver; but it was soon diminished far beyond the utmost limit of that process, so that very little of it remained; and examining this air, I found it to be but very little worse than that of the atmosphere, as that which came from the retort was a little better.

This experiment made it probable, that the air on the outside of the receiver had actually passed through it, only a little purified in its passage; and yet it was contrary to all the known principles of hydrostatics, and even any thing hitherto known in chemistry, that air should be transmitted through a vessel of this kind, and in a direction contrary to that in which it would have been forced by the pressure of the atmosphere; while the water, with which the clay was moistened, went the other way. For had the retort been pervious to air, as the inside had a free communication with the atmosphere, the water could not have risen within the receiver. This, however, appeared to be the case by the following decisive experiments.

Having filled the earthen retort with the moistened clay as before, I made the inside of the receiver perfectly dry, and placed it in a basin of mercury; when, upon heating the retort as before, the receiver was all covered with dew, which collecting into drops trickled down the inside of the receiver, and remained upon the mercury, which rose within the receiver, while air was received from the retort as usual. I had no doubt, therefore, but that all the water within the retort would have got through into the receiver. Spirit of wine, or something that had the smell of it, was transmitted from the clay through the retort in the same manner.

I then filled the receiver with inflammable air, and upon heating the retort it was all drawn through it, and delivered

as strongly inflammable as ever by the tube communicating with the inside of it, while the water rose within the receiver, and even covered the retort, which was fixed at the very top of it, so that hardly any of the inflammable air remained within it. In like manner nitrous air passed through the retort unchanged.

From these experiments it is impossible not to infer, that the clay of the earthen retort, being thus heated, destroys for a time the aerial form of whatever air is exposed to the outside of it; which aerial form it recovers after it has been transmitted in combination from one part of the clay to another, till it has reached the inside of the retort, while the water is drawn through it in the contrary direction.

Had this hypothesis been proposed *a priori*, it would, I doubt not, have been thought more extraordinary than the conversion of water into air. I propose to make many other experiments in the prosecution of these; but till I have an opportunity of doing this, I shall not trouble the Society with any conjectures that have occurred to me on the subject.

The great difficulty with respect to the experiment with the lens is, that the water should pass through the retort one way, and the air the other, and yet that the air should not be able to pass without the water. It is also not a little extraordinary, that the weight of the air and that of the water should be so nearly equal.

In the last place I must observe, that there is nothing in this experiment that contradicts the idea of the conversion of water into air, though it does not prove it: for still the experiment with the tobacco-pipe, in which the steam is made red-hot (whereas in that with the lens it is only of a boiling heat) cannot be explained so well on any other hypothesis any more

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than Mr. CAVENDISH's experiment on finding water on the decomposition of air.

I cannot conclude this account without acknowledging my obligation to Mr. PARKER for the use of his incomparable lens, and his obligingly assisting me in the management of it. Indeed, without this very instrument, or one of greater power than my own, I do not know that the last mentioned experiments could have been made at all; certainly not to so much satisfaction.

XXIII. *Description of an improved Air-Pump, and the Account of some Experiments made with it.* By Mr. Tiberius Cavallo, F. R. S.

Read July 3, 1783.

THE principal improvements which the air-pump received since it was first invented, were contrived by Mr. SMEATON, F. R. S. and are described in the XLVIIth volume of the Philosophical Transactions. This ingenious gentleman, considering the imperfections of the air-pumps usually made, not only found means to correct several of them, but improved almost every part of the machine, so as to render it incomparably superior to any thing of the kind done before.

It appears, by some accurate experiments of Mr. NAIRNE, F. R. S. described in the LXVIIth volume of the Phil. Transf. which were made with an air-pump constructed after Mr. SMEATON's principle, that by means of the best air-pumps made before Mr. SMEATON's invention, the rarefaction of the air within the receiver could never have been brought to more than forty or fifty times, if the heat of the place was about  $57^{\circ}$ ; that even with Mr. SMEATON's pump the receiver could not be exhausted beyond  $70^{\circ}$  or  $80^{\circ}$  of rarefaction, when moist leathers were used, or moisture was in any way introduced within the receiver; but that when this pump is quite free from moisture, and is newly cleaned, oiled, and put together, then.



then the air may by it be rarefied about 600 times, and not farther\*.

The principal cause which prevents this pump to exhaust farther than that limit is the weakened elasticity of the air remaining within the receiver, which, decreasing in proportion as the quantity of the air within the receiver is diminished, becomes at last incapable of lifting up the valve, which opens the communication between the receiver and the barrel; consequently no more air can in that case pass from the former to the latter.

To remove this principal imperfection of the best air-pumps had been attempted by several ingenious persons; but, as far as I know, was never obtained before the happy contrivance of the air-pump, which I am going to describe in the following pages.

Being in want of a good air-pump, and imagining that the opening and shutting the communication between the barrel and the receiver might, instead of the valve, be performed by means of a stop-cock, particularly constructed upon an idea of mine; I communicated my plan, about the latter end of the last year, to Mr. JACOB BARNARD HAAS, an ingenious workman in the philosophical-instrument way, who, in partnership with Mr. JOHN HENRY HURTER, had lately established a manufactory of philosophical instruments. Mr. HAAS remarked, that according to my plan the friction of the parts of

\* The degree of rarefaction shewed by what is called the *pear-gage*, when any vapour of water is within the receiver, is not to be considered as the degree of rarefaction of the elastic fluid in the receiver, but only of the air; for though the air may be exhausted, yet the vapour of water will supply its place; we shall, therefore, only take notice of the exhaustion when no vapour or moisture is within the receiver. See *HAIRNE's Experiments*, Phil. Trans. vol. LXVII.

the machine would be too great, and therefore the pump, if it answered at all, was not likely to be durable. In consequence of this, he considered upon some plan or other which might answer the same purpose in a better way, and soon hit upon a method of lifting up the valve at the bottom of the barrel, and of shutting it again at pleasure; which method being put into execution, has been found to answer exceedingly well.

Besides this capital improvement, his air-pump is rendered altogether more convenient for philosophical experiments, by answering several purposes, which will be enumerated after its description.

Plate I. shews a perspective view of this pump. Plate II. exhibits a geometrical delineation of its metal or working parts, detached from the wooden frame. And plate III. represents a section of its parts. The letters of reference are made to answer to the same parts in all the plates.

In plate III. fig. 1. AABI is the barrel of the pump, to the broad or flat ends of which are screwed, by means of five screws, the pieces CE and K, with leathers between, to render the junctures perfectly air-tight\*. The upper piece CE contains the collar of leathers for the handle or axis GG of the piston to go through, and the basin F, which serves for a cap to screw down the leathers, at the same time that it holds the oil, which may be put into it, in order to let the collar of leathers hold very tight; though it is shewn, by experience, that when the leathers are perfectly soaked, there is not the least occasion to keep any oil in the basin F. The same upper piece

\* All the leathers used for this pump are soaked in melted hog's lard; and when the parts of the instrument are put together, a little oil is smeared upon those surfaces of the brass pieces which go against the leather, though this oil may be spared.

CE contains the valve at E, which lets the air pass upwards, but prevents its return, and which is so contrived as that, when the piston is drawn quite to the top of the barrel, the least possible quantity of air should be left into the barrel. The parts which form this valve are shewn separately in fig. 3. where 1,3 is a brass piece that screws into a proper cavity made for its reception in the piece CE, and which is hollow, except its lower part, where it consists of a thin lamina perforated with a small hole 3. Into the hollow of the last mentioned part is screwed the other perforated piece 2,4, having a slip of oil-silk stretched over its lower part 4, and tied round a small indenture or groove made on its lower part. This slip of oil-silk answers better than a piece of bladder or leather: it just covers the hole 3, and is about four times broader than the diameter of the hole.

It will be easily conceived, that when the air is forced through the hole 3, it will lift up the slip of oil-silk, and passing by the sides of it, and also through the large perforation of the piece 2,4, will go upwards, &c.; but can by no means return backwards, since any pressure, that the air makes on the upper part of the oil silk, will only stop the passage more effectually.

A valve much like this is in the piston, the parts of which are shewn separately in fig. 4. *u* is a perforated brass piece screwed to the cylindrical handle or axis GG, which is also perforated with a short and bent hole. The piece *x* is screwed into the part *u*, and contains a valve, *viz.* a small piece 6 with a slip of oil-silk tied round its groove *yy*, which slip of oil-silk bears against the hole 5. The piece *x* screwing into the other piece *u*, fastens the round leathers which, about thirty in number, form the stopping part of the piston, and rub with their edges against the cavity of the barrel. This is a very useful improvement,

ment, since the common way of using two leathers turned over corks is both troublesome to make, and seldom fits exactly.

The piece K fastened to the lower end of the barrel is perforated with a hole, the direction of which is clearly seen in the figure, and which communicates with the perforation of the round piece L, which is screwed to K with a leather between. The perforation therefore of K communicates with the cavity of the brass tube RS, this being soldered to L. The part of the piece K, which projects within the barrel, is smaller in diameter than the cavity of the barrel, and the intervening space is exactly fitted by the moveable ring 8, 8, the two parts of which are screwed together, holding fast between; the edge of a piece of oil-silk, which stretches over the upper part of the piece K, and covers its aperture. A vertical view of the above-mentioned oil-silk, with five holes in it, is shewn in fig. 6.

It appears from this description, that the air can pass through the valve from without to within the barrel, but not *vice versa*. It will be also easily conceived, that the air can pass from the cavity of the tube R through the perforation of the pieces L and K, within the cavity of the barrel, only when the said air has elasticity, or force enough to push up the oil-silk. Now the principal improvement in this machine is, to lift up the oil-silk by a power applied externally, when the weakened elasticity of the air within the cavity of the tube R, &c. is not capable of doing it by itself, and here follows the description of this mechanism.

The double ring 8, 8, which holds the oil-silk, is fastened to two steel wires 9, 9, which are shewn in fig. 5.; this figure being a section of the lower part of the pump through a plane perpendicular to the plane of the section, fig. 1. Those wires

pass through collars of leathers held in proper brass boxes HQ, screwed to the piece K, and furnished with caps, 11, 11. The lower extremities of the wires are fastened to the cross bar, 7, 7, of the brass frame, OOO, a vertical view of which is shewn in fig. 4. of plate II.

From the middle of the piece L, fig. 1. pl. III. a pillar M proceeds, the lower part of which, branching into two horizontally, forms an axis  $z, z$ , fig. 4. pl. II. about which the brass frame OOO moves a little way upwards and downwards.

It appears from this description, that if the frame OOO fig. 4. pl. III. is moved upwards, the wires 9, 9, and likewise the double ring 8, 8, with the oil-filk, being all connected together, will be pushed also upwards; consequently, the oil-filk being removed from the hole of the piece K, a free communication is opened between the cavity of the tube R, and the cavity of the barrel, through which the air, however rarefied or weakened in elasticity, can pass without the least impediment.

In order to move the brass frame upwards, the end of a lever P bears against it. This lever is shewn in fig. 3. of plate II. which is a front view of part of the pump, whereas fig. 1. of the same plate is a side view of it. The center of motion of the lever is at 13, that is, between two side prominences of the piece 12, 12, which is fastened by screws to one of the wooden pillars of the frame of the machine. The part 15 of the lever, which projects beyond the wooden pillar, is made with a joint, by which means it may be turned upwards, as represented by 17, for packing it more conveniently.

When the valve is to be opened, the foot of the operator must press upon the extremity 15 of the lever, by which means the other extremity P, the frame OOO, the wires 9, 9, and the double ring 8, 8, with the oil-filk, are all lifted up. But in  
order

order to bring down again all those parts, and to shut the valve when the pressure of the foot is removed, there is an open brass tube N, fastened to the piece K, which contains a spiral spring, that, bearing against the extremity of the brass frame OO, pushes it downwards.

This principal mechanism being dispatched, it will be very easy to describe the remaining parts of this excellent machine: but before we proceed to that, I shall briefly describe another mechanism, which Mr. HAAS has lately contrived, to supply the place of that just mentioned, *viz.* of the lower part of the instrument, as being much more simple, and capable of answering the same purpose.

A section of this new thought, which he is going to execute for another pump, is shewn in fig. 7. of plate III.

AB is the lower extremity of the barrel. CCDE is a piece of brass perforated quite through with a large and cylindrical hole, and is screwed to the barrel instead of the piece K of fig. 1. with a leather between. On one side of this piece CDE, part of the surface is flattened, and to it is adapted, by means of screws and a leather, the piece of brass G, to which the tube H is soldered, which corresponds to the tube RS of fig. 1. The aperture of CC, towards the cavity of the barrel, is covered by a piece of oil-silk, like that represented in fig. 6. which is kept stretched by a brass ring LL, sunk into the piece CCD.

Within the cylindrical perforation of the piece CCDE, there is a long piston KIr, consisting of the following parts. IK is its axis, which spreads at top into a flat plate r, and the lower extremity of which is fastened at N to the lever MO, which moves round the center M. Towards the middle of the axis, there is a piece of brass, the shape of which is more easily

M m m 2

understood

understood by inspecting the figure, than by a verbal description, which piece confines the round leathers from itself to the plate *r*, and also from itself to the other plate, which is screwed upon the axis at K.

Between the last mentioned plate and the cap F, there is a spiral steel spring, which always pushes the piston upwards. Lastly, the axis of the piston is perforated from its top till towards the middle, where the perforation, opening side-way, communicates with the cavity of the tube H.

Now, when the piston is pushed upwards, as it always is when the extremity O of the lever is not pressed down, then the oil-silk at LL, laying against the plate *r*, covers the hole of the piston, consequently it shuts the communication between the barrel and the cavity of the tube H, &c. But if this communication is required to be opened, then the extremity O of the lever is pressed down, which will separate the upper part of the piston from the contact of the oil-silk, so as to open the communication as required.

Let us now return to the description of the other parts of the machine, as shewn in fig. 1. The upper extremity *o* of the tube RS is made conical, and is fitted by grinding into the strong brass piece of communication UX<sup>l</sup>; the cap T serving to tighten it\*. The extremity *n* of the opposite piece *np* is likewise fitted by grinding into the part *l*, and is tightened by the cap *m*. Just over the said conical extremities *n* and *o*, and into the same piece of communication UX<sup>l</sup>, are adapted two stop-cocks Z and *b*, which are tightened down by the caps

\* All the parts of this pump, that are fitted to each other by grinding, as the stop-cocks, the extremity of the tube RS, &c. before they are put together, are smeared with a mixture of bees-wax and oil melted together, in order to let them stop the better, and to prevent their wearing by friction.

Y and

Y and *i*; but as those stop-cocks must be turned by means of a key adapted to their square tops, and in that case their friction against the caps would unscrew or tighten them; therefore a ring is put round each cap, which ring is prevented from turning by a pin, and is fastened round the cap by means of a screw with a milled head. These rings are seen in fig. 1. of plate II. Each of the stop-cocks is perforated with a hole, which goes from one side to the bottom of it.

The upper part of the piece of communication terminates in a ball *a*, into which the lower and conical extremity *b* of the tube *d* is adapted by grinding, and is fastened by the cap *c*. The tube *d* is soldered to the part *e*, which is made fast to the upper board of the frame of the machine, and to which the plate *ff*, having the rim *gg*, is screwed with a leather between.

The lower part of the piece *np* is screwed with a leather between to the top of the strong brass vessel *qrs*, and is terminated as appears in the figure, for a reason that will be made evident in the sequel. The vessel *qrs* is fastened to the middle shelf of the wooden frame by screws *ss*; and it has a perforation at bottom, which is shut by the screw nut *t*, and serves to let out the oil, which, after working the machine for some time, will be found lodged in the vessel *qrs*.

The tube *Er* is soldered into the piece *D*, and likewise into the vessel mentioned above, wherein it proceeds till very near the top of the vessel, where it opens.

The piston of the pump, with its axis *GG*, is moved upwards and downwards by means of the rock-work, wheel, and handle; which parts being clearly shewn in fig. 1. and 2. of plate II. and also being nothing new, require no farther description.



To the fore-side of the ball *a*, on the strong piece of communication, is adapted by a cross-piece and a screw, the gage which shews the exhaustion of the pump. This gage may be seen in fig. 1. of plate II. It consists of an outward glass tube containing a little quicksilver; and of an inside tube, which, like a barometer, is filled with, and inverted into, the quicksilver of the large tube. The inside tube is supported at top by a spring socket. A small ivory scale, with divisions, encompasses the small tube, and swims upon the surface of the quicksilver in the large tube. By means of this scale the exhaustion of the pump may be begun to be measured, after that the air is rarefied at least thirty times.

To the other side of the ball *a*, *viz.* opposite to the gage, there is a screw-nut, which, by means of a leather, shuts a hole made in the said ball, and serves to open the communication between the receiver and the atmosphere. This nut, besides a milled head, has a square filed on it, to which a key may be applied, in order to open the screw more easily, and by degrees without jerks, which can hardly be avoided when the nut is opened by one's finger applied immediately to it. This nut is seen in plate I.

I shall forbear from any more prolix description of other gages that may be adapted to this pump, as also of other parts necessary for performing experiments with it, these things being very well known at present, and containing nothing new.

Now, as to the working of this pump, a bare inspection of fig. 1. in plate III. will shew, that by the action of the piston, when moved up and down into the barrel, the air will be exhausted from the cavity of the tube RS, of the ball *a*, tube *d*, and of course from the glass receiver that is put upon the plate *ff*; for when the piston, after being let down, is drawn upwards, the

the under part of the barrel remains without any air; consequently the valve at the bottom of the barrel, having no pressure on one side, will be pushed up by the air in the receiver, which expanding comes through the tube *RS*, and part of it passes into the barrel. Then the piston on being let down, the air passes through the valve of the piston, to the upper part of the barrel, and when afterwards the piston is drawn up, this air is forced through the valve at *E* into the tube *Dr*, from thence into the vessel *qs*, through the channel *pn*, and, lastly, it will be expelled into the atmosphere through the aperture *k*.

As some small quantity of oil is always necessary to be put into the pump, this oil, by the action of the piston, is brought, together with the air, towards the tube *Dr*, and would come out of the hole *k* if the vessel *qs* had not been placed to receive it; and it is for this reason, that the lower part of the piece *pn* is shaped as shewn in the figure, and that the tube *Dr* is made to proceed almost as far as the top of the vessel *qs*; for if the oil was permitted to come out of the aperture *k*, it would be scattered about the instrument and the operator, by the violence of the air coming out of *k*.

As this pump exhausts exactly in the same manner as other pumps do, the lever which opens the valve at the bottom of the barrel is not to be moved, except when such a degree of exhaustion is required as cannot be made by the instrument itself, *viz.* when worked in the ordinary way. In fact, it will be seen by the gage, that when the mercury cannot fall any lower by the usual way of working the pump, it will be instantly depressed by opening the valve at the bottom of the barrel, which evidently shews the great advantage of the improvement. In general, the lever may be begun to be pressed, or, which is the same thing, the valve to be opened, when the  
gage

gauge shews that the exhaustion of the air is to about 100 times, *viz.* that the quantity of the air remaining within the receiver is about the one-hundredth part of the air that was contained in it before the exhaustion was commenced. Care must be taken to open the valve only whilst the piston is drawing up, and to remove the foot from the lever the moment that the piston is impelled downwards, otherwise the work is useless.

In the situation in which the pump is shewn by fig. 1. pl. III. it is evident, that as the action of the pump determines the air to move from the tube RS into the barrel and from the barrel up the tube Dr, &c. ; it is evident, I say, that if a receiver is placed upon the plate *ff*, the air will be exhausted from it. But if the stop-cocks Z and *b* are so turned as that the side hole of the cock *b*, as well as that of the cock Z, is turned towards X, then, by the action of the pump, the air, instead of being exhausted, is condensed into the receiver properly placed upon the plate *ff*; for now the air coming from the atmosphere through the aperture X goes down the cavity of the tube RS, enters the barrel, and from the barrel is impelled upwards through Dr, through *pn*, through *a*, *b*, *d*, and lastly into the receiver, which in that case must be pressed down, as is usually done in condensing engines. This situation of the stop-cocks, *viz.* when the instrument is to be used as a condenser, is shewn in fig. 2. of plate III. Two letters, E and C, marked upon the square top of each cock, direct the operator how to turn them, whether for exhausting or for condensing.

The conical holes X and *k*, fig. 1. plate III. are made to receive occasionally the extremities of two stop-cocks. Those stop-cocks are fitted to the holes H and *k* by grinding, and a bladder is adapted to each of them. The use of this contrivance is to introduce into the glass receiver some particular sort of

of elastic fluid, or to receive into a bladder the elastic fluid that is contained in the receiver. Thus, suppose I want to introduce some fixed air into the receiver; first, I exhaust the common air from the receiver, then put the stop-cock of a bladder containing fixed air to the hole X; lastly I open the stop-cock of the bladder, and turn the cocks *bZ*, so that their side-holes may be turned towards X, and by working the pump the fixed air will immediately pass from the bladder into the receiver. If now this same fixed air is required to be introduced into a bladder again, the stop-cocks must be turned with their side holes towards *k*, and a stop-cock with an empty bladder is put to the hole *k*; then, by working the pump, the fixed air will be gradually introduced from the receiver into the bladder.

Having finished the description of this improved air-pump, which, besides its exhausting much better, has various other advantages over any other instrument of the kind; I shall conclude this paper with the succinct account of some experiments made with it, principally to determine how far it can rarefy the air.

*Experiments made with the above-described air-pump.*

Previous to the narrative of the experiments, it is necessary to mention, that both the plate, and the lower edges of the receivers of this pump, are ground so perfectly true, as not to require any leathers, nor even oil; however, for greater security, some oil is generally poured on the outside of the edge of the receiver, after having exhausted it a little; and it is very seldom, that any visible quantity of this oil passes within the receiver, between its edge and the plate of the pump.

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When the hole in the plate of the pump is stopped up by means of a screw and leather, and the instrument is worked for about three or four minutes, the quicksilver in the small tube of the gage falls so low as to be even with the quicksilver in the outside tube, which shews as if the air were entirely exhausted from the inside of the pump; but as it is difficult to judge whether the two surfaces of the mercury in the inside and outside tube are quite on the same level, and even if they were exactly so, there could always be suspected that a little air may be lodged within the gage, notwithstanding that several gages of this sort have been used, in some of which the mercury had been accurately boiled; we must, therefore, have recourse to other gages, in order to determine the exhausting power of this pump with more precision. Accordingly, the pear-gage and long barometer-gage were tried, the effects of which will be related in the sequel.

If, instead of stopping up the hole in the plate of the pump, a glass receiver is laid upon it, and the pump is worked, the gage will also come so low as when no receiver is put upon it; but it must be remarked, that after exhausting and leaving the instrument in that state, when no receiver is upon it, the quicksilver in the gage will be rising for about one hour, so as to ascend one-tenth, or at most one-fifth of an inch above the surface of the quicksilver in the outward tube, and then it remains stationary; whereas if the experiment be repeated when the receiver is upon the plate, the degree of exhaustion remains unaltered, the mercury not rising at all in the small tube. This rising of the quicksilver in the first case seems to be occasioned by some small quantity of elastic fluid, that is yielded by the oil contained between the parts of the machine; for this quantity of elastic fluid can occasion a sensible difference when the exhausted.

exhausted space within the pump is small, but it becomes quite inconsiderable when the receiver is upon the plate, its quantity bearing a very small proportion to the exhausted space.

It is of great advantage in this pump that very little oil can be lodged in it, because then the elastic fluid yielded by this fluid is in so small a quantity as not to affect the experiments. As for the oil, which by the working of the pump is accumulated into the oil vessel, it cannot interfere with the exhaustion of the pump, since it does not communicate with the cavity of the receiver.

The exhausting power of this pump was next examined with the pear-gage, made and placed under a receiver after the usual manner; and by this it appeared that the pump exhausted so far as to remain within the receiver less than the thousandth part of the air it contained before the exhaustion\*. In that case, the quicksilver in a short barometer-gage came to the same level in both tubes, which proves that, by this last mentioned gage in that state, the air is shewn to be rarefied at least one thousand times.

Lastly, a long tube, or what is called a long barometer-gage, was adapted to the pump by means of a bent brass tube. This glass tube went down along the side of the wooden frame, and its lower end was immersed in some quicksilver kept in a proper cistern.

On working the pump, when all the three gages were annexed to it, there appeared, that the quicksilver of the short gage came to the same level in the inside as well as outside tube,

\* Whenever the pear-gage was used, care was taken to keep the inside of the receiver, of the pear-gage, and of the pump, as free from moisture as it was possible.

that the quicksilver in the long barometer-gage came as high as it stood at in a real barometer, and that the pear-gage indicated a degree of rarefaction about one thousand. But as it was not known, whether the quicksilver had been boiled into the tube of the barometer, to which the long barometer-gage had been compared, therefore a barometer was accurately made for that purpose. The glass tube had been just drawn at the glass-house. It was perfectly clear; the quicksilver was boiled in the whole length of it, and care was taken that the dimensions of the tube, cistern, and divisions, were alike both in this barometer and in the gage. This done, the pump was tried again, and the quicksilver in the long gage rose within one-twentieth of an inch of the quicksilver in the barometer, or rather less, which shews that the air was rarefied little above 600 times; but at this time the pump was not in proper order for trying such nice experiments. It was leaky, and had not been taken to pieces and cleaned for above six weeks, in which time it had been frequently used, and continually left exposed to the dust of a working-shop: yet it shews, that in these unfavourable circumstances this pump can rarefy the air above 600 times.

Considering all the above-mentioned circumstances and experiments, I think it may be concluded, that this pump, when in good order, can rarefy the air about one thousand times.

I shall, lastly, conclude this paper with a summary account of several electrical experiments, which were made with this pump; reserving to give a more ample and circumstantial account of them for another opportunity.

When the air-pump was in good order, a glass receiver, which had a brass cap cemented to its upper aperture or neck, was laid upon the plate; then the end of the prime conductor of an electric machine was placed within half an inch of the cap

cap of the receiver; so that when the machine was in action, the electric fluid in the form of sparks went from the conductor to the brass cap, and when the receiver was exhausted, it passed from the cap to the plate of the pump through the receiver, illuminating its whole cavity. The more perfect the vacuum was made, the better conductor of electricity it became, and the electric light was more equably diffused in it; but it became by no means faint, even when the receiver was exhausted to the utmost, though the light changed appearance according as the receiver was more or less exhausted. Those appearances were as follows.

Degrees of rarefaction as shewn by the short gage.	Appearances of the electric light within the receiver.
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Air rarefied 40 times.	Light in large, long, but divided, streams.
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70.	Fine diffused light, of a white colour.
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80	Beautiful diffused light inclining to
100	red or purple, and filling the
400	whole receiver.

When the gage shewed the utmost degree of exhaustion.	A diffused light, filled equally every part of the cavity of the jar. It had hardly any reddish hue.
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In this state, or even when the air within the receiver was rarefied not above 100 times, if the brass cap of the receiver was made to communicate with the ground by means of good conductors in perfect contact, the light within the receiver occasioned by the electric sparks given by the prime conductor to the cap of the receiver was thereby diminished, but it did not



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not intirely vanish ; which shews, that the electric fluid, which proceeded from the conductor to the cap of the receiver in the form of sparks, did not pass to the earth all through the conductor, by which the cap was made to communicate with the ground ; but part of it went at the same time through the vacuum, so that when the pump in this experiment was insulated, sparks could be drawn from the plate of it.

Having repeated the above mentioned experiments with only this variation, *viz.* that the extremity of the prime conductor was put in contact with the cap of the receiver, the appearances of electric light within the receiver were very nearly the same as in the preceding experiments ; but if now the cap of the receiver was made to communicate with the ground, the light within the exhausted receiver vanished intirely, though the electrical machine acted very vigorously.

When a pith-ball electrometer was suspended within the receiver from its brass cap, and some electricity was communicated to the outside of the said cap, its balls diverged very little when the air within the receiver was rarefied about 100 times ; their repulsion was hardly discernible when the air was rarefied about 300 times ; but in a greater degree of rarefaction they did not diverge in the least, and that was the case whether a small or a large quantity of electric fluid was communicated to the cap of the receiver.

June 24, 1783.















XXIII. *Extract of a Letter from the Rev. James Augustus Hamilton, M. A. to the Rev. Nevil Maskelyne, D. D. F. R. S. giving an Account of his Observation of the Transit of Mercury over the Sun, of Nov. 12, 1782, observed at Cook's-Town, near Dungannon, in Ireland.*

Read April 20, 1783.

S I R,

Cook's-Town in Ireland,  
Nov. 16, 1782.

**F**ROM the very indulgent eye with which you regarded my astronomical wishes, when I had the pleasure of seeing you at Greenwich, I make no doubt of your pardon for offering my imperfect efforts towards observing the late transit of Mercury. I do not mean to trouble you with the perusal of the labours of the week, but only to state to you my general apparatus and results, and to request your kind communication of your observation of the same phænomenon, if it occurred at Greenwich. I have only a good common clock with a varnished deal pendulum, moving sidereal time; a transit-instrument, of four feet eight inches focal distance, with a triple object-glass, and the axis of two feet, on solid stone pillars, the base common to both, each pillar being a single stone of five feet high: the collimation is constantly attended to, and was examined a day or two preceding the transit by an observation of the \* polar star (the

\* The weather being hazy prevented the horizontal meridian mark being distinctly observable.



only circumpolar one my situation enables me to see an intire revolution of) and I found on inverting the axis that the error of collimation amounted to only  $2''$  of time at the polar star. By an observation of the sun's passage before the transit, the clock was slow  $-21'',7$ ; and by an observation of the passage of Lyra corrected strictly by your tables, after the ingress of Mercury, the clock appeared to be  $21'',4$  slow, its rate for the week losing  $1'',5$  per revolution. I observed with an achromatic tube of three inches aperture, triple object-glass, and used a magnifying power of about 90 times, which I preferred on account of the state of the atmosphere. At about two o'clock I set a stop watch to apparent solar time, and placed myself at the telescope within hearing of the beat of the transit-clock. I kept the part of the disk where I expected the ingress in constant view, my sight being directed by a vertical wire in the eye-tube, and at 2 h. 22' 3'' I stopped the watch, and counted 20'', to be sure of my having really perceived the first impression (which I apprehend could not have been shewn  $1''$  sooner by the power, &c. I used). I then stopped seconds to the clock, and counted up to an even minute, and found, that the first external contact happened at 17 h. 33' 11'' by the clock, or 2 h. 21' 45'' apparent time. Mercury came in like a distinct black point, without any preceding haziness or appearance of atmosphere; and at 17 h. 39' 10'' by the clock, or 2 h. 27' 43'' apparent time, the thread of light seemed completed, and then I date the internal contact. I had no instrument fit to take any micrometer measures, so continued only looking at the planet till the sun got so low, that the limb presented the appearance of a troubled sea at a distant horizon, among the waves of which Mercury once more plunged at about 18 h. 52', and the sun and planet

planet both left my view at about 18 h. 57'; but these observations are only good conjectures. From my best observations of eclipses of Jupiter's first satellite, of appulses of the moon's centre to the meridians, and lunar distances with a HADLEY's quadrant, I make my longitude 26' 35'' W. (nearly), and my latitude by a mean of many observations, is 54° 38' 20''.

I have the honour to be, &c.

XXV. *Methodus Inveniendi Lineas Curvas ex proprietatibus Variationis Curvaturæ. Auctore Nicolao Landerbeck, Mathes. Profess. in Acad. Upsalienfi Adjuncto: communicated by Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.*

Read March 13, 1783.

P A R S P R I M A.

**Q**UALITAS curvaturæ in diversis lineis diversisque earum punctis diversa reperitur. Circulo ubique eadem est curvatura, quæ in alia quavis curva, continue crescendo vel decrescendo, figuram ab uniformi circuli variat; quo enim majori velocitate progrediens crescit vel decrescit curvaturæ radius, eo citius curvæ a circuli osculatorii curvatura deflectit; et quo majori celeritate isochrona ipsa curva crescit vel decrescit, eo citius fertur motu angulari radius curvedinis et remotius idem curvaturæ gradus locum obtinet, quo circulus curvam osculans eam in angulo majori vel minori in puncto contactus simul secat. Hæc curvaturæ a circulari aberratio, quæ curvaturæ variatio nuncupatur, etsi alia in alia curva gaudeat proprietate, mensurari et exprimi potest generaliter per rationem fluxionum radii curvedinis et curvæ, quæ ratio proinde variationis index censenda est, ut in opere, quod Methodus Fluxionum inscribitur, illustrissimus NEWTONUS nos docuit. Demonstravit præterea MACLAURINUS in propositione trigesima sexta Tractatus de Fluxionibus, quod index hic variationis curvaturæ curvæ cujuscunque sit ut tangens anguli, linea punctum in curva et centrum curvaturæ evolutæ jungente et radio curvaturæ in isto puncto comprehensi; cujus analytica expressione, quæ pro quavis curva calculo differentiali facile habetur, intima curva-

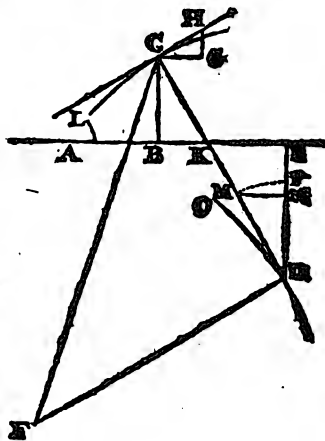
rum examinare licet, ut non solum punctum ejusdem curvæ, ubi inequabilitas curvaturæ est vel nulla vel datæ magnitudinis vel minima vel maxima vel infinita determinare, sed etiam curvas inter se comparare valeant matheſeos periti, ut quibus punctis curvatura ſit æqualis et ſimilis diſcernere queant. Methodum ex proprietatibus variationis curvaturæ inveniendi curvas explicatam adhuc non vidi, quæ, ſi detecta et explicata fuerit, quantum matheſeos ſcientiæ interſit, quemque præbeat uſum in problematibus tam mathematicis quam phyſicis ſolvendis, quæ a curvatura dependent, mathematicorum eſt judicare, quorum etiam judicio, quæ ad methodum hanc explicandam feci tentamina ſubjicio.

## THEOREM A. I.

Si curvæ cujusdam LC, ad axin concavæ vel convexæ, index variationis curvaturæ, seu tangens anguli DCF, radio curvaturæ CD in puncto C et linea CF, punctum C et centrum curvaturæ F evolutæ QD juagente, comprehensi, dicatur T, sinus anguli BCD  $p$ , posito sinu toto 1, arcus curvæ LC  $z$ , coordinatæ orthogonales AB, BC  $x$  et  $y$  earumque fluxiones  $dp$ ,  $dz$ ,  $dx$  et  $dy$  respective

dicantur, erit  $\frac{ddx}{dx} = -\frac{T dp}{\sqrt{1-p^2}}$ .

Sumatur DM unitati æqualis et ducantur DE axi AB et MN ipsi DE normales, et describatur arcus circuli MP; erit  $MN = p$  et  $DN = \sqrt{1 - p^2}$ . Quoniam ob similitudinem triangulorum DNM et CHG, erit  $DN (\sqrt{1 - p^2}) : MN (p) :: CG (dx) : GH (dy)$



( $dy$ ) et  $dy = \frac{pdx}{\sqrt{1-p^2}}$ , eamque ob caussam  $DN (\sqrt{1-p^2}) : DM$

(1) ::  $CG (dx) : CH (dz)$  et  $dz = \frac{dx}{\sqrt{1-p^2}}$ . Si radius curvaturæ

$CD$  fit  $R$  et ponatur constans, ejus enim fluxio ex coordinatarum non dependet, erit lineæ  $BE$  fluxio  $= -dx$ . Propter similitudinem triangulorum  $CBK$ ,  $KED$  et  $NDM$  erit  $DM (1) : MN$

( $p$ ) ::  $CK + KD (R) : BE = R p$ , cujus fluxio  $R dp = -dx$  et  $R = -\frac{dx}{dp}$ , et si hujus æquationis fluxiones sumantur, posita  $dp$

constante, habetur  $dR = -\frac{dix}{dp}$ , quæ per  $dz = \frac{dx}{\sqrt{1-p^2}}$  divisa dat

$$T \left( = \frac{dR}{dz} \right) = -\frac{ddx \sqrt{1-p^2}}{dx dp}, \text{ qua prodit } \frac{ddx}{dx} = -\frac{T dp}{\sqrt{1-p^2}}.$$

*Cor. 1.* Si tangens anguli  $BCD$  designetur per  $r$ , erit  $p = \frac{r}{\sqrt{1+r^2}}$ ,  $\sqrt{1-p^2} = \frac{1}{\sqrt{1+r^2}}$  et  $dp = \frac{dr}{1+r^2}$ , unde  $\frac{ddx}{dx} = -\frac{T dr}{1+r^2}$ .

*Cor. 2.* Si secans anguli  $BCD$  dicatur  $S$ , erit  $p = \frac{\sqrt{s^2-1}}{s}$ ,  $\sqrt{1-p^2} = \frac{1}{s}$  et  $dp = \frac{ds}{s^2 \sqrt{s^2-1}}$ , quo  $\frac{ddx}{dx} = -\frac{T ds}{s \sqrt{s^2-1}}$ .

*Cor. 3.* Si cosinus  $q$ , cotangens  $t$  et cosecans  $v$  dicantur, valores  $\frac{ddx}{dx}$  eandem habent formam, signis mutatis.

*Schol. 1.* Quum inventa sit  $T = -\frac{dix \sqrt{1-p^2}}{dx ap}$ , methodum habemus perfacilem calculandi generaliter variationem curvaturæ uniuscujusque curvæ; data enim relatione inter fluxiones coordinatarum, quæ per æquationem hujus formæ  $dy = X dx$  exhibetur, ubi  $X$  functio est abscissæ  $x$ , datur  $\frac{p}{\sqrt{1-p^2}} = X$ , qua  $x$  per  $p$  et  $p$  per  $x$  exprimi potest. Si variatio curvaturæ per  $p$  expressa desideretur, ponatur  $x = P$ , quantitatis  $p$  functioni, et fluxionibus

bus primis  $dx = P dp$  et secundis  $ddx = P' dp$ , posita  $dp$  constante, sumtis, valoribusque pro  $dx$  et  $ddx$  substitutis, habetur curvæ propositæ index variationis curvaturæ  $T = -\frac{P' \sqrt{1-p^2}}{P}$ , denotan-

tibus  $P'$  et  $P$  functiones quantitatis  $p$ . Si vero index variationis curvaturæ exprimenda fit per  $x$ , æquatione  $X = \frac{p}{\sqrt{1-p^2}}$  inveniatur

$p = X$  et  $\sqrt{1-p^2} = \sqrt{1-X^2}$ , sumtisque æquationis  $p = X$  primis et secundis fluxionibus,  $dp$  constante habita, erit  $dp = X' dx$  et  $0 = X'' ddx + X''' dx^2$ , qua  $ddx = -\frac{X''' dx^2}{X''}$ , et substitutione

debita  $T = \frac{X'' \sqrt{1-X^2}}{X'}$ , significantibus  $X''$ ,  $X'$ , et  $X$  functiones abscissæ  $x$ .

*Schol. 2.* Hoc adhibito theoremate inveniuntur curvæ, si inter  $T$  et  $p$ ,  $T$  et  $r$  vel  $T$  et  $s$  detur quædam relatio. Sit enim  $T = P$ , functioni quantitatis  $p$ , habetur  $\frac{ddx}{dx} = -\frac{P dp}{\sqrt{1-p^2}}$ , et facta integratione  $\log. dx = -\int \frac{P dp}{\sqrt{1-p^2}} + \log. A dp$ , quæ, si  $N$  fit numerus, cujus logarithmus hyperbolicus 1, evadit  $\log. dx = -\log. N \int \frac{P dp}{\sqrt{1-p^2}} + \log. A dp$ , et si  $N \int \frac{P dp}{\sqrt{1-p^2}}$  ponatur  $F$  et transeundo a logarithmis ad quantitates absolutas, erit  $dx = \frac{A dp}{F}$ , cujus si sumantur integralia, obtinetur  $x + C = \int \frac{A dp}{F}$ , qua equatione  $p$  per  $x$  exprimi possit. Sit  $p = X$ , functioni abscissæ  $x$ , erit  $\sqrt{1-p^2} = \sqrt{1-X^2}$ ,  $dy (= \frac{p dx}{\sqrt{1-p^2}}) = \frac{X dx}{\sqrt{1-X^2}}$  et integratione  $y = \int \frac{X dx}{\sqrt{1-X^2}}$  æquatio, qua curvarum natura innotescit.

Patet

Patet hinc, quod, quoties  $\int \frac{Pdp}{\sqrt{1-p^2}}$  per logarithmos sumi non possit, curva, quæ quæritur, sit transcendens; ut vero sit algebraica, requiritur, non solum ut  $\int \frac{Pdp}{\sqrt{1-p^2}}$  sit integrale logarithmicum, sed etiam ut  $\int \frac{Adp}{F}$  et  $\int \frac{Xdx}{\sqrt{1-X}}$  sint quantitates, quæ absolutam admittant æquationem.

*Exempl. 1.* Si invenienda sit curva, cujus variatio curvaturæ  $T = \frac{3\sqrt{1-p^2}}{p}$ . Per theorema habetur  $\frac{ddx}{dx} (= -\frac{Tdp}{\sqrt{1-p^2}}) = -\frac{3dp}{p}$ , quam æquationem integrando et corrigendo prodit  $\log. dx (= \log. \frac{1}{p^3} + \log. -\frac{adp}{2}) = \log. -\frac{adp}{2p^3}$ , et a logarithmis ad quantitates absolutas transeundo  $dx = -\frac{adp}{2p^3}$ , et iterum integrando et corrigendo  $x + C (= -\int \frac{a^{\frac{1}{2}}p}{2p^3}) = \frac{a}{4p^2}$ , ex qua æquatione habetur  $p = \frac{\sqrt{a}}{2\sqrt{C+x}}$  et  $\sqrt{1-p^2} = \frac{\sqrt{4C+4x-a}}{2\sqrt{C+x}}$ , unde sequitur, quod sit  $y (= \int \frac{pdx}{\sqrt{1-p^2}}) = \int \frac{\sqrt{a} \cdot dx}{\sqrt{4C+4x-a}} = \sqrt{a} \cdot \sqrt{4C+4x-a}$ , qua æquatione constat, curvam esse parabolam apollonianam, cujus parameter principalis  $a$ .

*Exempl. 2.* Si curva quæritur, cujus variatio curvaturæ  $T = \frac{1-3p^2}{p\sqrt{1-p^2}}$ , theoremate habetur  $\frac{ddx}{dx} (= -\frac{Tdp}{\sqrt{1-p^2}}) = \frac{3p^2-1}{p \cdot \sqrt{1-p^2}} \cdot \frac{dp}{p}$ , cujus æquatio integralis correctæ erit  $\log. dx (= \log. \frac{1}{p \cdot \sqrt{1-p^2}} + \log. adp) = \log. \frac{adp}{p \cdot \sqrt{1-p^2}}$ , vel, factæ a logarithmis transitu,  $\frac{dx}{a} = \frac{dp}{p \cdot \sqrt{1-p^2}}$  et integratione  $\frac{x}{a} + C = \log. \frac{p}{\sqrt{1-p^2}}$ , unde si  $N$  sit nu-

merus,

merus, cujus logarithmus hyperbolicus 1, erit  $\frac{p}{\sqrt{1-p^2}} = N^{\frac{x}{a}} + C$

et  $y (= \int \frac{p dx}{\sqrt{1-p^2}}) = \int N^{\frac{x}{a}} + C dx$ , curva igitur est logarithmica.

*Exempl. 3.* Si curvaturæ variatio fit  $T = \frac{3 \cdot \overline{a^2 + b^2} \cdot r}{a^2 r^2 \pm b^2}$ , quæritur curva. Per corollarium primum habetur  $\frac{dx}{dr} (= -\frac{T dr}{1+r^2})$   
 $= -\frac{3 \cdot \overline{a^2 + b^2} \cdot r dr}{a^2 r^2 \pm b^2 \cdot 1 + r^2}$  et integratione facta  $\log. dx (= \log. \frac{1+r^2}{a^2 r^2 \pm b^2})$   
 $+ \log. \pm \frac{b^2 a^2 dr}{2 \cdot 1 + r^2}) = \log. \pm \frac{b^2 a^2 dr}{2 \cdot a r \pm b^2}$ , vel, fumendo quantitates absolutas,  $\pm dx = \frac{b^2 a^2 dr}{2(a r \pm b^2)}$ , et integratione  $C \mp x =$   
 $\frac{a^2 r}{2 \sqrt{a^2 r^2 \pm b^2}}$ , ex qua æquatione  $r = \frac{b \cdot 2C \mp 2x}{a \sqrt{2C \mp 2x}^2 - a^2}$  et  $y (= \int r dx) =$   
 $\int \frac{b \cdot 2C \mp 2x \cdot dx}{a \sqrt{2C \mp 2x}^2 - a^2}$ , æquatio indolem curvarum exprimens, quæ  
 si  $C = \frac{a}{2}$  erit  $y = \frac{b \sqrt{ax \mp x^2}}{a}$ , æquatio pro sectionibus conicis.

*Exempl. 4.* Proponatur invenire curvam, cujus curvaturæ variatio  $T = \frac{2 \cdot 2p^2 - 3}{p^2 - 2 \cdot \sqrt{1-p^2}}$ , per secantem anguli BCD expressa, datur. Per corollarium secundum curvam consequi licet; sed per substitutionem  $T = \frac{2 \cdot 3p^2 - 1 \sqrt{1-p^2}}{p \cdot 2p^2 - 1}$  habetur, erit  $\frac{ddx}{dx}$   
 $(= + \frac{T dp}{\sqrt{1-p^2}} = \frac{2 \cdot 1 - 3p^2 \cdot dp}{p \cdot 2p^2 - 1})$ , integratione  $\log. dx (= \log. \frac{1}{p^2 \sqrt{2p^2 - 1}} + \log. adp) = \log. \frac{adp}{p^2 \sqrt{2p^2 - 1}}$  et adhibendo quantitates ab-

solutas



solutas  $dx = \frac{a^2 p}{p^2 \sqrt{2p^2 - 1}}$  cujus æquatio integralis  $x + C = \frac{a \sqrt{2p^2 - 1}}{p}$

dat  $p = \frac{a}{\sqrt{2a^2 - x + C^2}}$  et  $\sqrt{1 - p^2} = \frac{\sqrt{a^2 - x + C^2}}{\sqrt{2a^2 - x + C^2}}$ , quo  $y (= \int \frac{p dx}{\sqrt{1 - p^2}})$

$= \int \frac{adx}{\sqrt{a^2 - x + C^2}}$  æquatio pro curva, quæ finuum vocatur.

## THEOREMA II.

Si cofinus anguli BCD sit  $q$ , posito radio 1, et reliquæ determinationes maneant ut in theoremate præcedenti, erit

$$\frac{dy}{dy} = \frac{Tdq}{\sqrt{1 - q^2}}.$$

Nam propter triangulorum DMN et CHG similitudinem  $MN(\sqrt{1 - q^2}) : DN(q) :: HG(dy) : CG(dx)$  et  $MN(\sqrt{1 - q^2}) : MD(1) :: HG(dy) : CH(dz)$  erit  $dx = \frac{qdy}{\sqrt{1 - q^2}}$  et  $dz = \frac{dy}{\sqrt{1 - q^2}}$ .

Per similitudinem triangulorum CDK, KED, et NDM, erit  $MD(1) : DN(q) :: DK + KC(R) : y + DE$ , unde  $Rq = y + DE$ ,

sumptisque fluxionibus  $Rdq = dy$ , quæ  $R = \frac{dy}{dq}$ , radius enim curva-

turæ ut constans suppositus, DE etiam constans erit, et si ulterius fumantur fluxiones,  $dq$  constante habita, erit  $dR =$

$\frac{ddy}{dq}$ , quæ divisa per  $dz = \frac{dy}{\sqrt{1 - q^2}}$  provenit  $T (= \frac{dR}{dz}) = \frac{ddy\sqrt{1 - q^2}}{dydq}$  et

$$\frac{dy}{dy} = \frac{Tdq}{\sqrt{1 - q^2}}.$$

Cor. I. Si cotangens anguli BCD dicatur  $t$ , erit  $q = \frac{t}{\sqrt{1 + t^2}}$ ,

$$\sqrt{1 - q^2} = \frac{1}{\sqrt{1 + t^2}}, \quad dq = \frac{dt}{1 + t^2} \quad \text{et} \quad \frac{dy}{dy} = \frac{Tdt}{1 + t^2}.$$

Cor.

Cor. 2. Si cosecans anguli BCD fit  $v$ , erit  $q = \frac{\sqrt{v^2 - 1}}{v}$ ,  
 $\sqrt{1 - q^2} = \frac{1}{v}$ ,  $dq = \frac{dv}{v^2 \sqrt{v^2 - 1}}$  et  $\frac{dy}{dy} = \frac{T dv}{v \sqrt{v^2 - 1}}$ .

Schol. 1. Si per æquationem hujus formæ  $dx = Y dy$ , ubi  $Y$  functio est ordinatæ  $y$ , relatio datur inter coordinatarum fluxiones æquatione  $T = \frac{dy \sqrt{1 - q^2}}{dy dq}$ , eodem calculandi modo ac in scholio 1.

variatio curvaturæ  $T = \frac{Q \sqrt{1 - q^2}}{Q}$  generaliter in  $q$  habetur, signifi-

cantibus  $Q$  et  $Q$  functiones cosinus  $q$ . Pari calculandi ratione ac in eodem Scholio curvaturæ variatio  $T = -\frac{Y \sqrt{1 - Y^2}}{Y}$ , deno-

tantibus  $Y$ ,  $Y$  et  $Y$  functiones ordinatæ  $y$ , inveniri potest.

Schol. 2. Per hoc theorema natura curvæ habetur ex data relatione inter  $T$  et  $q$ ,  $T$  et  $r$  vel  $T$  et  $s$ , &c. Nam si sit  $T = Q$ , functioni cosinus  $q$ , erit  $\frac{dy}{dy} = \frac{Q dq}{\sqrt{1 - q^2}}$ , et integratione  $\log. dy = \int \frac{Q dq}{\sqrt{1 - q^2}} + \log. B dq$ , vel  $\log. dy = \log. N \int \frac{Q dq}{\sqrt{1 - q^2}} + \log. B dq$ , si  $N$  sit numerus, cujus logarithmus hyperbolicus 1; et si  $N \int \frac{Q dq}{\sqrt{1 - q^2}}$  dicatur  $G$ , et facto a logarithmis transitu, prodit  $dy = \frac{B dq}{G}$ , et per integrationem  $y + C = \int \frac{B dq}{G}$  ex qua  $q$  in  $y$  datur. Sit  $q = Y$ , functioni ordinatæ  $y$ , erit  $\sqrt{1 - q^2} = \sqrt{1 - Y^2}$  et  $x (= \int \frac{q dy}{\sqrt{1 - q^2}})$   
 $[= \int \frac{Y dy}{\sqrt{1 - Y^2}}]$  generalis æquatio, indolem curvarum exprimens.

Ad hæc idem est observandum ac in theoremate præcedenti, quod si  $\int \frac{Qdq}{\sqrt{1-q^2}}$  integrale fit logarithmicum et  $\int \frac{Bdp}{G}$  et  $\int \frac{Ydy}{\sqrt{1-Y^2}}$  quantitates perfecte integrabiles, curva evadit algebraica, si vero aliter evenierit, semper transcendens.

*Ex. 1.* Propositum esto invenire curvam, cujus variatio curvaturæ  $T = \frac{1}{q\sqrt{1-q^2}}$ . Per theorema habetur  $\frac{dy}{dy} (= \frac{Tdq}{\sqrt{1-q^2}}) = \frac{dq}{q \cdot 1-q^2}$ , integratione et correctione peracta,  $\log. dy (= \log. \frac{q}{\sqrt{1-q^2}} + \log. -adq) = \log. -\frac{aqdq}{\sqrt{1-q^2}}$ , et adhibendo quantitates absolutas  $dy = -\frac{aqdq}{\sqrt{1-q^2}}$ , et denuo integrando erit  $y+C (= -a \int \frac{q dq}{\sqrt{1-q^2}}) = a\sqrt{1-q^2}$ , unde  $\sqrt{1-q^2} = \frac{y+C}{a}$  et  $q = \frac{\sqrt{a^2 - y+C^2}}{a}$  et  $x (= \int \frac{q dy}{\sqrt{1-q^2}}) = \frac{dy\sqrt{a^2 - y+C^2}}{y+C}$  et si  $C=0$  pro venit  $x = \int \frac{dy\sqrt{a^2 - y^2}}{y}$ , qua constat, curvam esse tractoriam.

*Ex. 2.* Quænam est curva, cujus curvaturæ variatio  $T = \frac{3q^2-2}{q\sqrt{1-q^2}}$ ? Vi theorematism habetur  $\frac{dy}{dy} (= \frac{Tdq}{\sqrt{1-q^2}}) = \frac{3q^2-2 \cdot dq}{q \cdot 1-q^2}$ , integratione et correctione  $\log. dy (= \log. \frac{1}{q\sqrt{1-q^2}} + \log. -adq) = \log. -\frac{adq}{q\sqrt{1-q^2}}$ , hoc est  $dy = -\frac{adq}{q\sqrt{1-q^2}}$ , et iterum integrando  $y+C (= -a \int \frac{dq}{q\sqrt{1-q^2}}) = \frac{a\sqrt{1-q^2}}{q}$ , qua habetur  $\frac{q}{\sqrt{1-q^2}} = \frac{a}{y+C}$  et  $x (= \int \frac{q dy}{\sqrt{1-q^2}}) = \int \frac{ady}{y+C}$ , et si  $C=0$ ,  $x = \int \frac{dy}{y}$  æquatio pro logarithmica ordinaria.

THEOREM

THEOREMA III.

Manentibus iisdem ac in theoremate primo, erit  $\frac{ddx}{dz} = -\frac{Tdp}{\sqrt{1-p^2}}$   
vel etiam  $\frac{ddx}{dz} = \frac{Tdq}{\sqrt{1-q^2}}$ .

Est enim  $dx = \frac{dx}{\sqrt{1-p^2}}$  et  $dx = dz\sqrt{1-p^2}$ , quare  $R (= -\frac{dx}{dp})$   
 $= -\frac{dz\sqrt{1-p^2}}{dp}$ , cujus fluxiones  $dR = -\frac{ddx\sqrt{1-p^2}}{dp}$ , posita arcus  
MP fluxione  $\frac{dp}{\sqrt{1-p^2}}$  constante, per  $dz$  divisæ dant  $T (= \frac{dR}{dz})$ .  
 $= -\frac{ddx\sqrt{1-p^2}}{dzdp}$ , qua sequitur  $\frac{ddx}{dz} = -\frac{Tdp}{\sqrt{1-p^2}}$ . Et quum fluxio  
arcus circuli æqualis sit negativæ fluxioni complementi, erit etiam  
 $\frac{ddx}{dz} = \frac{Tdq}{\sqrt{1-q^2}}$ .

Cor. Si sint ut antea tangens anguli BCD,  $r$  et secans  $s$ , ha-  
betur  $\frac{ddx}{dz} = -\frac{dr}{1+r^2} = -\frac{ds}{s\sqrt{s^2-1}}$ .

Schol. 1. Si alterutra æquationum formæ  $dx = Zdx$  et  $dy = Zdx$ , inter fluxiones abscissæ vel ordinatæ et curvæ, relatio  
detur, per formulam  $T = -\frac{ddx\sqrt{1-p^2}}{dzdp}$  vel  $T = \frac{ddx\sqrt{1-q^2}}{dzdq}$ , va-  
riatio curvaturæ in  $p$ ,  $-\frac{p\sqrt{1-p^2}}{p}$ , in  $q$   $\frac{q\sqrt{1-q^2}}{q}$ , et in  $z$   $\frac{Z\sqrt{1-p^2}}{Z}$ ,  
eodem ac antea habetur, posita fluxione quantitatis  $\int \frac{dp}{\sqrt{1-p^2}}$  con-  
stante.

Schol. 2. Ope hujus theorematism invenire licet indolem curvæ,  
si inter  $T$  et  $p$ ,  $T$  et  $q$ , &c. relatio detur. Sit  $T = P$ , functioni  
P p p 2 sinus

sinus  $p$ , erit  $\frac{ddz}{dz} = -\frac{Pdp}{\sqrt{1-p^2}}$ , facta integration et correctione debita,  $\log. dz = -\int \frac{Pdp}{\sqrt{1-p^2}} + \log. \frac{Edp}{\sqrt{1-p^2}}$ , vel  $\log. dz = -\log. N \int \frac{Pdp}{\sqrt{1-p^2}} + \log. \frac{Edp}{\sqrt{1-p^2}}$ , si  $N$  fit basis logarithmorum hyperbolicorum, atque posita  $N \int \frac{Pdp}{\sqrt{1-p^2}} = H$ , et facto de logarithmis transitu,  $dz = \frac{Edp}{H\sqrt{1-p^2}}$ , et iterum integrando  $z + C = \int \frac{Edp}{H\sqrt{1-p^2}}$ , unde  $p$  per  $z$  habetur. Sit  $p = Z$ , functioni arcus curvæ  $z$ , erit  $\sqrt{1-p^2} = \sqrt{1-Z^2}$ ,  $x (= \int dz \sqrt{1-p^2}) = \int dz \sqrt{1-z^2}$  et  $y (= \int p dz) = \int Z dz$ , quorum alterutra curvarum indoles cognoscitur. Pari modo procedendum est, si  $T = Q$ , quantitas  $q$  functioni.

Hinc facile colligitur, quod, quoties  $\int \frac{Pdp}{\sqrt{1-p^2}}$  fit integrale logarithmicum et quantitates  $\int \frac{Edp}{H\sqrt{1-p^2}}$  et  $\int dz \sqrt{1-Z^2}$  vel  $\int Z dz$  perfectæ integrabiles, curvæ erunt rectificabiles et algebraicæ, quoties ratio inter  $x$  et  $z$  vel inter  $y$  et  $z$  in relationem algebraicam  $x$  et  $y$  resolvi possit.

*Exempl. 1.* Si desideretur curva, ejus curvaturæ variatio  $T = \frac{2\sqrt{1-p^2}}{p}$ . Per theorema est  $\frac{ddz}{dz} (= -\frac{Tdp}{\sqrt{1-p^2}}) = -\frac{2dp}{p}$  et integration  $\log. dz (= \log. \frac{1}{p^2} + \log. \frac{adp}{\sqrt{1-p^2}}) = \log. \frac{adp}{p^2\sqrt{1-p^2}}$ , qua  $dz = \frac{a/p}{p^2\sqrt{1-p^2}}$ , et denuo integrando  $z + C = -\frac{a\sqrt{1-p^2}}{p}$ , qua ha-

betur

betur  $p = \frac{a}{\sqrt{a^2 + z + C}}$ ,  $\sqrt{1 - p^2} = \frac{z + C}{\sqrt{a^2 + z + C}}$  et  $x (= \int dz \sqrt{1 - p^2}) = \frac{z + C \cdot dz}{\sqrt{a^2 + z + C}}$ ; si  $C = 0$ , evadit  $x (= \int \frac{z dz}{\sqrt{a^2 - z^2}}) = -a + \sqrt{a^2 - z^2}$ , curva igitur est catenaria.

*Exempl. 2.* Sit variatio curvaturæ  $T = \frac{\sqrt{1 - q^2}}{q}$ , quæritur curva. Vi theorematis erit  $\frac{dz}{dx} (= \frac{T dq}{\sqrt{1 - q^2}}) = \frac{dq}{q}$  et integratione  $\log. dz (= \log. q + \log. \frac{dq}{\sqrt{1 - q^2}}) = \log. \frac{aq dq}{\sqrt{1 - q^2}}$ , qua  $dz = \frac{aq dq}{\sqrt{1 - q^2}}$  et rursus integrando  $z + C = -a\sqrt{1 - q^2}$ , unde  $q = \frac{\sqrt{a^2 - z + C^2}}{a}$ ,  $\sqrt{1 - q^2} = \frac{z + C}{a}$  et  $y (= \int dz \sqrt{1 - q^2}) = \int \frac{z + C}{a} \cdot dz$ , si  $C = -a$  patet curvam esse cycloidem.

#### THEOREMA IV.

Retentis antea adhibitis denominationibus, erit  $\frac{dR}{RT} = - \frac{dp}{\sqrt{1 - p^2}}$ .

Quoniam  $DM (I) : CD (R) :: - \frac{dp}{\sqrt{1 - p^2}} : dz$  habetur  $dz = - \frac{R dp}{\sqrt{1 - p^2}}$ , quæ æquatio per  $T$  multiplicata dat  $T dz = - \frac{RT dp}{\sqrt{1 - p^2}}$ , et quum  $dR = T dz$ , prodit  $\frac{dR}{RT} = - \frac{dp}{\sqrt{1 - p^2}}$ .

*Schol. 1.* Hujus theorematis subsidio inveniri potest curvarum indoles, si inter  $R$  et  $T$  detur quædam relatio. Sit  $R = K$ , quantitatis  $T$  functioni, habetur per hoc theorema  $\frac{dK}{KT} = - \frac{dp}{\sqrt{1 - p^2}}$ ,  
et

et facta integratione  $\int \frac{dK}{KT} + C = -\int \frac{dp}{\sqrt{1-p^2}}$ . Quoniam  $-\int \frac{dp}{\sqrt{1-p^2}}$  arcus est circuli, cujus sinus  $\sqrt{1-p^2}$ , si ponatur  $\int \frac{dK}{KT} + C = n$  et  $N$  numerus, cujus logarithmus hyperbolicus 1, erit  $\sqrt{1-p^2} = \frac{N^{\sqrt{-1}} - N^{-\sqrt{-1}}}{2\sqrt{-1}}$ , functioni quantitatis  $T$ , unde per hanc æquationem  $T$  in  $p$  vel substitutione  $T$  in  $q$  vel  $r$ , &c. exprimi potest. Cognita relatione inter  $T$  et  $p$  vel  $T$  et  $q$ ,  $r$ , &c. relationem inter coordinatas vel inter curvam et abscissam vel ordinatam per theoremata præcedentia inveniendi aditus patet.

Hinc facile colligitur, quod quoties  $\int \frac{dK}{KT}$  non fit per arcus circulares integrabilis curva semper fit transcendens.

*Ex. 1.* Quænam est curva, si relatio inter  $R$  et  $T$  per æquationem  $R = \frac{a \cdot 4 + T^2}{4}$  detur. Theorematis auxilio erit  $\frac{2dT}{4+T^2} (= \frac{dR}{RT}) = -\frac{dp}{\sqrt{1-p^2}}$  et integratione  $\int \frac{2dT}{4+T^2} + C = -\int \frac{dp}{\sqrt{1-p^2}}$ , ubi  $\int \frac{2dT}{4+T^2}$  arcus est circuli, cujus sinus  $\frac{T}{\sqrt{4+T^2}}$  et  $-\int \frac{dp}{\sqrt{1-p^2}}$  arcus, cujus sinus  $\sqrt{1-p^2}$ , si arcus constantis  $C$  sinus sit  $c$ , erit  $\frac{T\sqrt{1-p^2} + 2C}{\sqrt{4+T^2}} = \sqrt{1-p^2}$ , qua æquatione  $T$  in  $p$  invenire licet. Si  $C=0$ , habetur in hoc casu speciali  $T = \frac{2\sqrt{1-p^2}}{p}$  et per theorema 1.  $dy = \frac{adx}{\sqrt{2ax+x^2}}$ , curva igitur quæsitæ est catenaria.

*Ex. 2.* Quæritur curva, si  $R = \frac{a\sqrt{1+4T^2}}{2}$ . Vi theorematis obtinetur  $-\frac{2dT}{1+4T^2} (= \frac{dR}{RT}) = \frac{dq}{\sqrt{1-q^2}}$  et integrando  $-\int \frac{2dT}{1+4T^2} + C =$

$= \int \frac{dq}{\sqrt{1-q^2}}$ . Itaque quum arcuum  $= \int \frac{2dT}{1+4T^2}$  et  $\int \frac{dq}{\sqrt{1-q^2}}$  sinus sint  $\frac{1}{\sqrt{1+4T^2}}$  et  $q$  respective, si arcus constantis  $C$  sinus sit  $c$ , prodit  $\frac{\sqrt{1-C^2} + 2CT^2}{\sqrt{1+4T^2}} = q$ , qua  $T$  in  $q$  habetur. Si  $C=0$ , erit  $T = -\frac{\sqrt{1-q^2}}{2q}$  et per theorema 2. prodit  $dx = -\frac{y^2 dy}{\sqrt{a^4 - y^4}}$ , unde constat, quod in hoc casu curva sit elastica.

# THEOREMA V.

Manentibus adhibitis denominationibus et dicta  $DF$ ,  $S$ , erit  $\frac{ds}{ST} - \frac{dT}{T^2} = -\frac{dp}{\sqrt{1-p^2}}$ .

Quoniam  $1 : T :: CD (R) : DF (S)$ , erit  $S=RT$  et  $R = \frac{S}{T}$  ejusque fluxiones  $dR = \frac{dS}{T} = \frac{SdT}{T^2}$ . Quum vero  $\frac{dR}{RT} = -\frac{dp}{\sqrt{1-p^2}}$ , prodit substitutione  $\frac{ds}{ST} - \frac{dT}{T^2} = -\frac{dp}{\sqrt{1-p^2}}$ .

*Schol.* Mediante hoc theoremate indagantur curvæ, data relatione inter  $S$  et  $T$ . Si enim sit  $S=L$ , quantitatis  $T$  functioni, habetur  $\frac{TdL-LdT}{LT^2} = -\frac{dp}{\sqrt{1-p^2}}$  et integratione  $\int \frac{TdL-LdT}{LT^2} + C = -\int \frac{dp}{\sqrt{1-p^2}}$ . Ponatur  $\int \frac{TdL-LdT}{LT^2} + C = m$  et  $N$  basis logarith-

morum hyperbolicorum, erit  $\sqrt{1-p^2} = \frac{N^{m\sqrt{-1}} - N^{-m\sqrt{-1}}}{2\sqrt{-1}}$ , quæ functio est quantitatis  $T$ , quare  $T$  in  $p$  vel substitutione in  $q$ ,  $r$ , &c. per hanc æquationem exprimi potest. Relatione adepta inter  $T$  et  $p$  vel  $q$ , &c. relatio inter coordinatas, vel inter curvam et abscissam vel ordinatam habetur, ut antea expositum est.



Generaliter constat, quod, quoties  $\int \frac{TdL - LdT}{LT^3}$  non sit pars arcus circulares integrabilis, curva sit transcendens.

Ex. 1. Si radius curvaturæ evolutæ  $S = \frac{aT \cdot \sqrt{9+T^2}}{54}$ , quaeritur curva. Per theorema obtinetur  $\frac{3dT}{9+T^2} (= \frac{dS}{ST} = \frac{dT}{T^2} = -\frac{dp}{\sqrt{1-p^2}})$  et integratione  $\int \frac{3dT}{9+T^2} + C = -\int \frac{dp}{\sqrt{1-p^2}}$ . Quum vero arcuum  $\int \frac{3dT}{9+T^2}$  et  $-\int \frac{dp}{\sqrt{1-p^2}}$  finis sint  $\frac{T}{\sqrt{9+T^2}}$  et  $\sqrt{1-p^2}$ , si arcus constantis C finis fit c, erit  $\frac{T\sqrt{1-p^2} + 3C}{\sqrt{9+T^2}} = \sqrt{1-p^2}$  et resoluta hac æquatione T in p habetur. Si fit  $c=0$ , erit  $T = \frac{3\sqrt{1-p^2}}{p}$  et per theorema 1.  $y = \sqrt{ax}$ , curva igitur in hoc casu est parabola Apolloniana.

Ex. 2. Quænam est curva, si evolutæ curvaturæ radius  $s = \frac{aT \cdot \sqrt{9+4T^2}}{2\sqrt{27}}$ ? Theoremate habetur  $\frac{6dT}{9+4T^2} = -\frac{dp}{\sqrt{1-p^2}}$  et integratione  $\int \frac{6dT}{9+4T^2} + C = -\int \frac{dp}{\sqrt{1-p^2}}$ . Arcuum  $\int \frac{6dT}{9+4T^2}$  et  $-\int \frac{dp}{\sqrt{1-p^2}}$ , finis sunt  $\frac{2T}{\sqrt{9+4T^2}}$  et  $\sqrt{1-p^2}$ , si arcus constantis C finis ponatur c, prodit  $\frac{2T\sqrt{1-p^2} + 3C}{\sqrt{9+4T^2}} = \sqrt{1-p^2}$ , per quam T in p obtinetur, quæ, in casu  $c=0$ , dat  $T = \frac{3\sqrt{1-p^2}}{2p}$  et theoremate 1.  $dy = \frac{a^2 dx}{\sqrt{x^2 - a^2}}$  æquatio ad curvam, quæ construitur rectificatione ellipsoes et hyperbolæ æquilateræ conjunctim.

THEOREMA VI.

Dicatur CF, U et reliquis manentibus, erit  $\frac{dU}{UT} - \frac{dT}{1+T^2} = -\frac{dp}{\sqrt{1-p^2}}$ .

Quum enim  $1 : \sqrt{1-T^2} :: CD (R) : CF (U)$ , erit  $R = \frac{U}{\sqrt{1+T^2}}$  ejusque fluxio  $dR = \frac{dU}{\sqrt{1+T^2}} - \frac{UTdT}{1+T^2}$ , et quum  $\frac{dR}{RT} = \frac{dp}{\sqrt{1-p^2}}$ , provenit substitutione  $\frac{dU}{UT} - \frac{dT}{1+T^2} = -\frac{dp}{\sqrt{1-p^2}}$ .

Schol. Auxilio hujus theorematis, curvæ inveniuntur, quando inter T et U ratio detur. Nam si fit  $U = M$ , functioni quantitatis T, erit per hoc theorema  $\frac{1+T^2 \cdot dM - MTdT}{MT \cdot 1+T^2} = -\frac{dp}{\sqrt{1-p^2}}$ .

et integratione  $\int \frac{1+T^2 \cdot dM - MTdT}{MT \cdot 1+T^2} + C = -\int \frac{dp}{\sqrt{1-p^2}}$ . Itaque,

posita basi logarithmica N et  $\int \frac{1+T^2 \cdot dM - MTdT}{MT \cdot 1+T^2} + C = k$ , erit

$\sqrt{1-p^2} = \frac{N^k \sqrt{-1} - N^{-k} \sqrt{-1}}{2 \sqrt{-1}}$ , quantitatis T functioni, quare inter

T et p habetur relatio, per quam, methodo antea exposita, relationem inter coordinatas vel curvam et abscissam sive ordinatam invenire licet.

Consequitur hinc, quod, quando  $\int \frac{1+T^2 \cdot dM - MTdT}{MT \cdot 1+T^2}$  per quadraturam circuli non obtinetur, curva semper sit transcendens.

Ex. Si curva quaeritur ubi linea CF sive  $U = \frac{a}{2}$ , theorematis

ope erit  $-\frac{dT}{1+T^2} = \frac{dq}{\sqrt{1-q^2}}$  et integratione  $-\int \frac{dT}{1+T^2} + C = \int \frac{dq}{\sqrt{1-q^2}}$ .

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Quum

Quum arcuum  $-\int \frac{dT}{1+T^2}$  et  $\int \frac{dq}{\sqrt{1-q^2}}$  sinus sint  $\frac{1}{\sqrt{1+T^2}}$  et  $q$  si arcus constantis  $C$  sinus sit  $c$ , obtinetur æquatio  $\frac{\sqrt{1-c^2}+CT}{\sqrt{1+T^2}} = q$ , qua  $T$  in  $q$  datur, et si  $c=0$ ,  $T = \frac{\sqrt{1-q^2}}{q}$ , quare in hoc casu speciali per theorema 2. habetur  $dx = -\frac{2\sqrt{y} \cdot dy}{\sqrt{a-2y}}$ , æquatio pro cycloide ordinaria cujus circuli generatoris diameter  $\frac{a}{4}$ .

## THEOREMA VII.

Si variatio curvaturæ evolutæ dicatur  $V$  ceteris manentibus, erit  $\frac{dT}{V-T \cdot T} = -\frac{dp}{\sqrt{1-p^2}}$ .

Quoniam  $DM (1) : CD (R) :: -\frac{dp}{\sqrt{1-p^2}} : dx$ , habetur  $dx = -\frac{Rdp}{\sqrt{1-p^2}}$ , quæ si multiplicetur per  $T$  prodit  $dR (=Tdx) = -\frac{RTdp}{\sqrt{1-p^2}}$ , et propter  $1 : T :: CD (R) : DF$  erit evolutæ radius curvaturæ  $DF = RT$ , cujus fluxio  $RdT + TdR$  per fluxionem evolutæ divisa dat ejus curvaturæ variationem  $V (= \frac{RdT}{dR} + T)$   $= -\frac{dT\sqrt{1-p^2}}{Tdp} + T$  atque inde  $\frac{dT}{V-T \cdot T} = -\frac{dp}{\sqrt{1-p^2}}$ .

*Schol.* Hoc mediante theoremate invenire valemus curvas, si inter curvaturæ variationes  $V$  et  $T$  ratio detur. Sit enim  $V=H$ , functioni quantitatis  $T$ , erit vi theorematis  $\frac{dT}{H-T \cdot T} = -\frac{dp}{\sqrt{1-p^2}}$  et integrando  $\int \frac{dT}{H-T \cdot T} + C = -\int \frac{dp}{\sqrt{1-p^2}}$ , si itaque ponatur  $\int \frac{dT}{H-T \cdot T} + C = l$  et  $N$  basis logarithmica, erit  $\sqrt{1-p^2} =$

$\frac{N\sqrt{-1} - N^{-1}\sqrt{-1}}{2\sqrt{-1}}$ , qua æquatione  $T$  in  $p$  vel substitutione in  $q, r$ , &c. exprimi potest, unde via, æquationem ad curvam inveniendi, patet.

Curva semper est transcendens, quoties  $\frac{dT}{H-T \cdot T}$  per circuli rectificationem non habetur.

*Exempl.* Sit evolutæ variatio curvaturæ  $V = T + \sqrt{T^2 - 4}$ , quæritur curva. Theoremate hoc habetur  $\frac{dT}{T\sqrt{T^2-4}} (= \frac{dT}{H-T \cdot T}) = \frac{dq}{\sqrt{1-q^2}}$  et integratione  $\int \frac{dT}{T\sqrt{T^2-4}} + C = \int \frac{dq}{\sqrt{1-q^2}}$  arcus, quorum sinus sunt  $\frac{\sqrt{T + \sqrt{T^2-4}}}{\sqrt{2T}} c$ , et  $q$ , si arcus constantis  $C$  sinus ponatur  $c$ , et exinde consequitur  $\frac{\sqrt{1-c^2}\sqrt{T + \sqrt{T^2-4}} + c\sqrt{T - \sqrt{T^2-4}}}{\sqrt{2T}} = q$ , qua si  $c=0$  prodit  $T = \frac{1}{q\sqrt{1-q^2}}$  et per theorema 2.  $dx = \frac{dy\sqrt{a^2-y^2}}{y}$  in quo casu curva est tractoria:

XXVI. *A Series of Observations on, and a Discovery of, the Period of the Variation of the Light of the bright Star in the Head of Medusa, called Algol. In a Letter from John Goodricke, Esq. to the Rev. Anthony Shepherd, D. D. F. R. S. and Plumian Professor at Cambridge.*

Read May 15, 1783.

S I R,

York, May 12, 1783.

**I** TAKE the liberty to transmit to you the following account of a very singular variation in Algol or  $\beta$  Persei, which you will oblige me by presenting to the Royal Society, if you think it deserving that notice. All that has been hitherto known concerning the variation of this star, as far as I can find after the most diligent researches, is comprised in the following passage in DU HAMEL's *Historia Regiæ Scientiarum Academiæ, liber IV. sect. 6. caput VIII. de rebus Astronomicis, ann. 1695, p. 362.* "Id quoque testatur D. MONTANARI stellam lucidiorem " Medusæ, diversis annis, variæ esse magnitudinis: nullam pene " in eâ mutationem potuit advertere D. MARALDI annis 1692 " et 1693; sed anno 1694 aucta est et imminuta insigniter, modo " quarti, modo tertii, modo secundi ordinis stella apparuit." This, however curious, is only a very vague and general information; but the following observations, lately made, exhibit a regular and periodical variation in that star, of a nature hitherto, I believe, unnoticed.

The first time I saw it vary was on the 12th of November 1782, between eight and nine o'clock at night, when it appeared of about the fourth magnitude; but the next day it was of the second magnitude, which is its usual appearance. On the 28th of December following, I perceived it to vary again thus; at 5 $\frac{1}{2}$  h. in the evening, it was about the fourth magnitude, as on the 12th of November last; but at 8 $\frac{1}{2}$  h. I was much surprized to find it so quickly increased as to appear of the second magnitude. My friend Mr. EDWARD FIGOTT, whom I informed of this singular phænomenon as soon as I saw it, also observed it; and I had the pleasure to find that his observations coincided with mine. The subsequent observations which I have made on this star are very particular; and I think it will be best to give a brief extract of them in their order from my journal; but it is necessary I should first specify the usual and greatest magnitude of Algol, as also the relative brightness and magnitude of those stars to which I compared it during the progress of its variation.

The usual and greatest magnitude of Algol is this; of the second magnitude, much less bright than  $\alpha$  Persei, and not so much as  $\gamma$  Andromedæ; brighter than  $\alpha$  Cassiopeæ and  $\beta$  Arietis, and nearly the same, if not rather brighter, than  $\alpha$  Pegasi and  $\beta$  Cassiopeæ; not quite so bright as  $\gamma$  Cassiopeæ, and much brighter than  $\epsilon$  Persei and  $\beta$  Trianguli. The relative brightness of the stars to which I compared it during the progress of its variation is as follows;  $\alpha$  Cassiopeæ is the brightest, and of near the second magnitude;  $\beta$  Arietis is the next, and of between the second and third magnitude; then  $\epsilon$  Persei and  $\beta$  Trianguli, both of the third magnitude;  $\zeta$  Persei is somewhat less bright than  $\epsilon$  Persei, and also of the third magnitude;  $\delta$  Persei is less than  $\zeta$  Persei, and rather of between the third and

and fourth magnitude;  $\epsilon$  Persei, which Algol is equal to at its least brightness, is not so bright as  $\delta$  Persei, and of about the fourth magnitude.

## OBSERVATIONS ON ALGOL.

*Brightness and magnitude of Algol.*

January 14, 1783.

At 6 h. it was varied from its usual brightness, but rather brighter than  $\beta$  Arietis.

At  $6\frac{1}{4}$  h. equal to  $\beta$  Arietis, but rather a little less bright, and of between the second and third magnitude.

At  $7\frac{1}{4}$  h. third magnitude; not so bright as  $\beta$  Arietis, and equal to  $\beta$  Trianguli.

At  $7\frac{3}{4}$  h. nearly the same as at  $7\frac{1}{4}$ , but thought it was rather less bright than  $\beta$  Trianguli.

At  $8\frac{1}{4}$  h. between the third and fourth magnitude; not quite so bright as  $\beta$  trianguli, and rather less than  $\epsilon$  and  $\zeta$  Persei, but a little brighter than  $\delta$  and  $\rho$  Persei.

At  $9\frac{1}{4}$  h. about the fourth magnitude, and equal to  $\rho$  Persei.

The weather was cloudy till  $11\frac{1}{4}$  h. when it appeared to be of the third magnitude; much brighter than  $\rho$  Persei, and rather brighter than  $\gamma$  Persei.

At  $12\frac{1}{4}$  h. between the second and third magnitude, and brighter than  $\zeta$  and  $\epsilon$  Persei and  $\beta$  Trianguli.

January 17.

At  $7\frac{1}{4}$  h. it was of the third magnitude, equal to  $\epsilon$  Persei, and rather less than  $\beta$  Trianguli.

At 8 h. a very little brighter than  $\epsilon$  Persei, and equal to  $\beta$  Trianguli.

At

At 8½ h. rather brighter than  $\beta$  Trianguli, but the sky was not favourable.

January 31.

At 10½ h. varied from its usual brightness, but with some doubt.

At 11¼ h. certainly less bright; much less than  $\gamma$  Andromedæ, but brighter than  $\zeta$  and  $\epsilon$  Persei, and of between the second and third magnitude.

At 12¼ h. third magnitude, and rather brighter than  $\zeta$  and  $\epsilon$  Persei.

At 13 h. about the brightness of  $\zeta$  Persei, and much brighter than  $\epsilon$  Persei; but the sky was not favourable.

At 14½ h. about the fourth magnitude, and equal to  $\epsilon$  Persei, but afterwards increased.

February 6.

At 5½ h. it was rather a little brighter than  $\beta$  Arietis, and between the third and fourth magnitude.

At 6¼ h. about the third magnitude; not so bright as  $\beta$  Arietis, but brighter than  $\beta$  Trianguli and  $\epsilon$  Persei.

At 6½ h. about the same brightness as  $\beta$  Trianguli and  $\epsilon$  Persei.

At 7 h. between the third and fourth magnitude; not quite so bright as  $\beta$  Trianguli, nearly equal to  $\delta$  Persei, and a little brighter than  $\epsilon$  Persei.

At 7½ h. about equal to  $\epsilon$  Persei, and nearly of the fourth magnitude; but the sky was not favourable.

At 8 h. rather a little less bright than  $\epsilon$  Persei; sky still unfavourable.

At 8½ h. between the third and fourth magnitude rather a little brighter than  $\delta$  Persei, and a little brighter than  $\epsilon$  Persei.

At 9 h. certainly brighter than  $\delta$  Persei, and of the third magnitude.

At



At  $9\frac{1}{2}$  h. of the same brightness as  $\epsilon$  Persei; but the sky was not favourable.

At 10 h. brighter than  $\epsilon$  Persei.

At  $10\frac{1}{2}$  h. brighter than at 10 h. and of between the second and third magnitude.

At  $11\frac{1}{2}$  h. very bright; and now, as I think, at its usual magnitude.

On the 9th of February, at  $6\frac{1}{2}$  h. I thought it was less bright, and nearly equal to  $\beta$  Arietis; but have some doubts on account of the unfavourable sky.

February 23.

At  $10\frac{1}{2}$  h. it was brighter than at  $9\frac{1}{2}$  h. when I observed it at its usual brightness; now of the third magnitude, rather brighter than  $\epsilon$  and  $\zeta$  Persei.

At 11 h. about the same brightness as  $\epsilon$  and  $\zeta$  Persei.

At 12 h. between the third and fourth magnitude; not so bright as  $\epsilon$  and  $\zeta$  Persei, a little brighter than  $\eta$  Persei, and a little less than  $\delta$  Persei.

February 26.

At  $6\frac{1}{2}$  h. between the second and third magnitude; rather less bright than  $\alpha$  Cassiopeæ, but was not very certain.

At  $9\frac{1}{2}$  h. little less bright than  $\eta$  Persei, and of the fourth magnitude.

At 10 h. nearly between the third and fourth magnitude; a little brighter than  $\eta$  Persei, and a little less bright than  $\delta$  Persei.

March 1.

At  $8\frac{1}{2}$  h. it was of the third magnitude; a little brighter than  $\epsilon$  and  $\zeta$  Persei.

At  $8\frac{1}{2}$  h. brighter than at  $8\frac{1}{2}$  h.

At

At  $9\frac{1}{2}$  h. between the second and third magnitude; a little less bright than  $\alpha$  Cassiopeæ.

At 10 h. I believe it now at its usual brightness.

March 18, at  $9\frac{1}{2}$  h. Mr. B. PIGOTT thought it less bright; but the weather was very hazy.

March 21.

At  $7\frac{1}{2}$  h. it was about between the third and fourth magnitude; not so bright as  $\delta$  Persei, but brighter than  $\epsilon$  Persei.

At 8 h. rather a little brighter than  $\epsilon$  Persei, and sometimes equal to it.

At  $8\frac{1}{2}$  h. about the fourth magnitude; equal to  $\epsilon$  Persei; but sometimes it appeared rather a very little brighter.

At 9 h. rather a little brighter than  $\epsilon$  Persei.

At 10 h. about the third magnitude; equal to  $\zeta$  and  $\iota$  Persei, but rather a little brighter.

At  $10\frac{1}{2}$  h. brighter than  $\zeta$  and  $\iota$  Persei.

At 11 h. much brighter than  $\zeta$  and  $\iota$  Persei; rather between the second and third magnitude.

April 10.

At 8 h. it was about the third magnitude, and rather brighter than  $\iota$  Persei.

At  $8\frac{1}{2}$  h. nearly equal to  $\iota$  Persei, though rather a little brighter.

At 9 h. rather less bright than  $\iota$  Persei, but brighter than  $\delta$  Persei.

At  $9\frac{1}{2}$  h. rather less bright than  $\delta$  Persei, and between the third and fourth magnitude.

At 10 h. about the fourth magnitude; not so bright as  $\delta$  Persei, but brighter than  $\epsilon$  Persei.

At 10 h. rather less than at  $9\frac{1}{2}$  h.; believe it now very near its least brightness.

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April

April 13.

At 8 h. it was between the third and fourth magnitude; brighter than  $\epsilon$  Persei, but not so bright as  $\delta$  Persei.

At 8½ h. rather brighter than  $\delta$  Persei, and not so bright as  $\epsilon$  Persei.

At 9 h. rather brighter than  $\epsilon$  Persei. It was too low to observe its farther variation.

May 3.

At 9½ h. nearly between the third and fourth magnitude, and somewhat brighter than  $\epsilon$  Persei; but so low that I could not well compare it with other stars, or be able to observe the remainder of the variation. I believe it must have passed its least brightness not long before.

The times of the above observations are nearly apparent time, and were for the most part made under favourable circumstances. My friend Mr. EDWARD PIGOTT, to whom I am under great obligations on this as well as on other occasions, also observed some of the variations; and where our times of observation were the same, always agrees with me.

From an attentive comparison of all the particulars in the above observations it appears, first, that this star changes from the second to about the fourth magnitude in nearly three hours and a half, and from thence to the second magnitude again in the same space of time; so that the whole duration of this singular variation is only about *seven hours*. And, secondly, it appears also, that this variation probably recurs about every *two days and twenty-one hours*. This last conclusion will be rendered more conspicuous by the following table; the first column of which shews the days, and exact time of the day, when Algol was observed to be very near, or at its least brightness; the second column marks the different intervals of time

elapsed between the several observations; the third exhibits the quotient arising from a division of these intervals by a certain number of revolutions, each of two days and twenty-one hours, which number of revolutions are expressed in the last column.

The day and time when Algol was observed at or near its least brightness.			The different in- tervals between the several ob- servations.		The quotients of the divisions of the 2d column by the 4th.		Number of revolu- tions.
d.	h.		d.	h.	d.	h.	
1782 Nov.	12	8½					
Dec.	28	5½	45	21	2	20,8	16
1783 Jan.	14	9½	17	3½	2	20,6	6
	31	14½	17	5	2	20,8	6
Feb.	6	8	5	17½	2	21,	2
	23	12+	17	4	2	20,6	6
	26	9½	2	21½	2	21,5	1
Mar.	21	8½	22	23	2	20,9	8
April	10	10+	20	1½	2	20,8	7
April	13	8	2	22	2	22,*	1
May	3	9½	20	1	2	20,7	7

The results in the third column agree so nearly, that there is the greatest probability, not to say certainty, that the singular and quick variation of this star, during the space of seven hours, as above mentioned, recurs regularly and periodically about every two days and nearly twenty hours and three quarters.

To ascertain this period with greater accuracy and precision will require more time and observation: but I can add, that I

\* The difference of upwards of an hour in this quotient will easily be reduced to the others by remarking, that Algol was observed on the 10th and 13th of April, not when it was *at*, but only *near*, its least brightness: and, indeed, all the little differences of the rest will vanish by making a reasonable allowance of the same kind.

R r r 2

have

have constantly observed Algol, at different times, every night when the weather permitted, ever since the 28th of December last; and upon accurately examining all these observations in my journal, I find, that so far from containing any appearances the least contrary to the above conclusion, they strongly corroborate it, since I never observed that star varied in any of those days which, according to that theory, were the intervals between its variations. All Mr. EDWARD PIGOTT's observations, even at different times from mine, tend to confirm the same conclusion.

Whether this singular phenomenon is always the same; or whether it occurs only some years, and ceases intirely in others (as may be presumed from the account of MONTANARI and MARALDI above quoted); and whether in this case it recurs in regular periods of time or otherwise; are curious objects of investigation, which can only be determined by a long and regular course of observations for many years.

If it were not perhaps too early to hazard even a conjecture on the cause of this variation, I should imagine it could hardly be accounted for otherwise than either by the interposition of a large body revolving round Algol, or some kind of motion of its own, whereby part of its body, covered with spots or such like matter, is periodically turned towards the earth. But the intention of this paper is to communicate facts, not conjectures; and I flatter myself that the former are remarkable enough to deserve the attention and farther investigation of astronomers.

I am, &c.



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# E R R A T A

I N

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Page.	Line.	for	read
120	1	near FL. 19.	FL. 20.
	30	60° 55'	61° 23'
127	8	FL. 108. In seq. flexu 5 <sup>a</sup>	FL. 107. In seq. flexu 4 <sup>a</sup>
	17	Posit. 28° 17'	Posit. 31° 3'
130	15	FL. 1.	near FL. 1.
133	24	1' 7" 49'''	57" 49'''
130	24	† Capricorni, FL.	FL. 64. Sagittarii
139	8	2 ad α Cygni, FL. 45.	3 <sup>a</sup> ad α Cygni, FL. 46.
	12	3 ad α Cygni, FL. 46.	3 <sup>a</sup> ad α Cygni (FL. 46 <sup>a</sup> ) adjacent
	18	In Constell. Ceti.	In Constell. Ceti, FL. 66.
140	5	FL. 44.	FL. 43 <sup>a</sup> præcedens ad boream
144	21	Posit. 80° 47'	Posit. 79° 51'
145	3	Posit. 49° 58'	Posit. 5° 26'
146	22	near FL. 28.	A. FL. 28.
149	22	0 northwards	0 southwards
150	18	FL. 35.	FL. 41.
	26	FL. 59.	FL. 56. and 57.
151	16	73° 29'	75° 21'
153	11	Posit. 33° 42'	Posit. 61° 23'
154	7	2' 6" 12'''	2' 41" 46'''
	27	1' 5" 10'''	59" 4'''
156	4	FL. 4.	FL. 4.
161	21	6' 27"	6" 27'''

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303. 1. *for possible read* fusible  
 405. 6. *for these read* those  
 406. 5. *after phlogisticated add* air  
 407. 28. *before cobalt add* of  
 408. 1. penult. *for necessarily read* necessary  
 412. 6. *for equally read* equably  
 413. 8. *after dephlogisticated add* air  
 418. 8. *after imagined add* that  
 421. 13. *for come read* came  
 423. 8. *delo a before, water*  
 475. 7. *for 5 $\frac{1}{2}$  h. read 5 $\frac{1}{2}$ h.*  
 — 24. *for not quite so bright as  $\gamma$  Cassiopeæ read* rather less bright than  
      $\gamma$  Cassiopeæ.  
 476. 18. *for between the third and fourth magnitude read* between the second  
     and third magnitude.

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